THE

MATHEMATICAL GAZETTE

EDITED BY

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19 KIDMORE ROAD, CAVERSHAM, READING

LONDON

G. BELL AND SONS, LTD., PORTUGAL STREET, KINGSWAY

VOL. XIX. FEBRUARY, 1935. No. 232.

THE MATHEMATICAL ASSOCIATION.

THE Annual Meeting of the Mathematical Association was held at the Institute of Education on 7th-8th January, 1935.

On Monday, 7th January, the proceedings opened at 2.15 p.m. with the transaction of business; the President, Professor E. H. Neville, was in the chair. The Report of the Council * for the year 1934 was presented and adopted, and the Treasurer's statement of the financial position received. It was proposed that Rule 13 (iii) and Rule 17 (iii) should be altered to read thus:

Rule 13 (iii). The President shall retire after holding office for *one* year, and shall not be eligible for re-election as President for the ensuing year.

Rule 17 (iii). The number of Officers, other than the President and Vice-Presidents, and of unofficial members of the Council, shall not together exceed *thirteen*, of whom at least *eight* shall be persons residing within easy access of London.

An amendment to read *fourteen* in place of *thirteen* in the proposed form of Rule 17 (iii) was carried, and the alterations, thus amended, were carried. On the nomination of the Council, Mr. A. W. Siddons was elected President for the year 1935, and Professor Neville was elected a Vice-President. Miss R. H. King, Miss D. M. Clark and Mr. A. S. Gosset Tanner retired from the Council, and on a ballot Mr. J. H. Hope, Mr. A. W. Reilly and Miss L. M. Swain were elected to fill the vacancies. The following † were elected Honorary Members of the Association: Professors Borel, Hadamard, Hardy, D. E. Smith and Whittaker.

Professor Neville then delivered his Presidential address: *The Food of the Gods* ‡. This was followed by Brigadier H. St. J. L. Winterbotham's lecture, *Geography and Mathematics*.

On Tuesday, 8th January, four papers were read in the morning: *The Work of a Junior Mathematical Association* §, by Mr. G. L.

* See pp. 2-3. † See p. 4. ‡ Pp. 5-17. § To be published later.

Parsons; *The Solution of Triangles given Three Sides* *, by Mr. W. Hope-Jones; *The Orthocentric Simplex in Space of Three and Higher Dimensions* *, by Mr. H. Lob; *The Use of Signs in Geometry* *, by Mr. H. V. Mallison. The afternoon meeting began with Professor D. R. Hartree's paper, *The Bearing of Statistical and Quantum Mechanics on School Work* *, followed by a discussion on *The First Encounter with a Limit* *; the final item was a lecture by Professor G. H. Hardy on *The Theorem of the Arithmetic and Geometric Means* *.

A Publishers' Exhibition was open during the two days.

REPORT OF THE COUNCIL FOR THE YEAR 1934.

SINCE the last Annual Meeting, 191 new members have been admitted to the Association. The number of members now on the roll is 1422, of whom 3 are Honorary members, 95 are life members by composition, 5 are life members under the old rule, and 1319 are ordinary members.

The Council regrets to have to announce the deaths of the following members of the Association: Miss Lizzie Binden, the Rev. J. Cullen, Mr. H. S. Hall, Sir Horace Lamb, Sir Donald MacAlister, Bart., Miss M. K. Mitchener, Sir Thomas Muir, C.M.G., and Professor D. M. Y. Sommerville.

Sir Donald MacAlister had been a member of the Association for 53 years, and at the time of his death was Chancellor of the University of Glasgow. Sir Thomas Muir, formerly Superintendent-General of Education and Vice-Chancellor of the University of the Cape of Good Hope, was the last survivor of the original founders of this Association and had retained his membership for 64 years. Mr. H. S. Hall, formerly of Clifton College, had been a member for 57 years and Sir Horace Lamb for 52 years. Professor D. M. Y. Sommerville had been Professor of Pure and Applied Mathematics in Victoria University College, Wellington, New Zealand, since 1915.

The Teaching Committees.

The new **General Teaching Committee** has considered a draft report on the correlation of mathematics and science teaching, which had been prepared by a committee of the Science Masters' Association; some suggestions for its amendment were sent to the scientists and were adopted by them. A sub-committee was appointed to deal with a reference from the Board of Education; the memorandum which this sub-committee prepared was published in the October *Gazette*. The Examinations Sub-Committee has been reappointed.

The **Boys' Schools Committee** has been working during the year at the Supplementary Report on the Teaching of Geometry, to which reference was made in the last Report of the Council. Considerable progress has been made.

The **Girls' Schools Committee** has held two meetings during the year. The questions of mathematics for the non-specialist and of the requirements of Training Colleges were discussed.

* To be published later.

The Problem Bureau.

There has been no falling off in the number or quality of the problems submitted to the Bureau during 1934. Requests for advice as to details in teaching and as to the choice of textbooks are on the increase, but in reply it is always explained that the advice offered is personal, not official.

The Branches.

The formation of the Branches Committee has made possible a closer relationship between the scattered Branches, to the great benefit of the Association as well as of the Branches themselves.

All the Branches report a successful session and useful meetings. The subject of most widespread interest has naturally been the Algebra Report issued with the July *Gazette*; many of the Branches are devoting a meeting to its discussion. Particulars of meetings of the Branches are announced in the *Gazette* insets. Two papers read to the London Branch have been published in the *Gazette*, and the Yorkshire Branch has continued its practice of issuing a full independent Report of its proceedings.

The composition of the Branches in terms of Members and Associates is as follows: London, 203, 99; Yorkshire, 54, 92; Manchester, 38, 104; Bristol, 12, 26; Midland, 30, 40; Liverpool, 17, 58; South-West Wales, 11, 30; North-Eastern, 45, 14; Southampton, 2, 24; Cardiff, 22, 38; Queensland, 11, 14; Sydney, 19, 122; Victoria, 7, 8. The London Branch has become stronger in both categories in the course of the year, while the Manchester and Midland Branches have added very substantially to their numbers of Associates.

The Council.

Professor E. H. Neville has now completed his year of office as President of the Association and the Council desires to express its most grateful appreciation of the manner in which he has continued to devote himself to the promotion of the Association's well-being. He has added much to the debt which the Association already owed him for his many years' service in other offices and on the Council.

The Council nominates Mr. A. W. Siddons, of Harrow School, for election as President of the Association for the year 1935, and Professor Neville for election as a Vice-President.

The Council desires also to express the cordial thanks of the Association to Mr. T. A. A. Broadbent, to Professor E. H. Neville and Mr. F. Beames, and to Mr. A. S. Gosset Tanner, for the invaluable services which they have continued to render in the Editorship of the *Gazette*, in the administration of the Library, and in the Directorship of the Problem Bureau respectively; also to Miss R. H. King, Miss D. M. Clark and Mr. A. S. Gosset Tanner, who now retire from the Council, for their services as members of the Council since the years 1930, 1931 and 1931 respectively.

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OUR NEW HONORARY MEMBERS.

E. Borel, Professeur de calcul des probabilités et de physique mathématique à la Sorbonne, is perhaps the best-known French mathematician of the day. Not only is he famous for his own researches and for the school which he has formed, but he is known to the whole mathematical world as the editor, inspirer and part author of two encyclopaedic works, the collection of monographs on the theory of functions and the *Traité du Calcul des Probabilités*.

J. Hadamard, Professeur de mécanique analytique et de mécanique céleste au Collège de France, gave fresh inspiration to workers in the theory of numbers by his proof in 1893 of the famous prime number theorem; since then his researches have advanced the boundaries of mathematics at many points. He is the present President of the International Commission on the Teaching of Mathematics, a body whose influence on the teaching of mathematics is appreciated by every member of the Mathematical Association.

G. H. Hardy, Sadleirian Professor of Pure Mathematics in the University of Cambridge, is a past President of the Association. His profound researches into the most subtle points of analysis may seem to him his most important work, but members of the Association will couple with these his many activities for the improvement of mathematical education in this country, in particular as the destroyer of the order of merit in the old Tripos and as the author of the most influential English treatise on pure mathematics of this century.

D. E. Smith, Professor Emeritus, Columbia University, New York, the author of a standard history of mathematics, of many mathematical textbooks and works on the teaching of mathematics, and a leading member of the International Commission, has on these grounds alone a strong claim to be regarded as an ideal honorary member of our Association, but we should not forget his interest in the relation of mathematics to that humanistic culture so often regarded as the Mathematical Antipodes.

E. T. Whittaker, Professor of Pure Mathematics in the University of Edinburgh, is, like Professor Hardy, a past President of the Association; like Professor Hardy, he, too, is at once the profound researcher, the lucid author, and the inspirer of a very lively school of mathematicians. His discovery of a general solution of Laplace's equation and his work on the functions associated with the elliptic cylinder indicate that part of mathematics which perhaps most interests him, though no part is to him foreign territory. His *Modern Analysis* and *Analytical Dynamics* are classics.

THE FOOD OF THE GODS.

By Prof. E. H. Neville

Presidential Address to the Mathematical Association, January 1935

A popular gibe against the branch of the teaching profession to which most of you belong is that the qualifications on which an appointment is made are not always related very closely to the duties of the post which is being filled. It is athletic prowess, we are told, that really matters. If only a man has a blue, all that is expected of him in the way of learning is sufficient intelligence to keep a clear week ahead of his class, in whatever subject he is engaged to teach. I am not going to ask whether there is or ever has been any truth in this aspersion, but I can properly point out to you this afternoon that you have no right to resent it. The accidents and aptitudes that may bring a man to the Chair of your Association are many and diverse, but one consideration is certainly never raised. You do not ask whether your President is likely to be able to perform the one function inseparable from his office, that is, whether on the one occasion in the year when you have the public ear, he will have anything to say that is worthy of the opportunity, and to-day you have no reason to hope that I can either interest you or entertain you for an hour. However, I am encouraged by remembering that some of the most valued members of the Association may fairly be described as sportsmen who have made good, and with their example to inspire me, I will do my best.

If I have succumbed to temptation and followed precedent to give my address a cryptic title, I hope that none of you anticipate an hour's talk on mathematics in general. Mathematics has been called by a vast number of fanciful names, and I daresay " the food of the gods " is among these, but the application I have to make of the phrase is to a very definite problem, which impressed itself on me when I discovered some months ago, to my sincere surprise, that if I had sat for an entrance scholarship at Cambridge within the last year or two I should have had a fair prospect of success. Do not misunderstand me. I am not boasting childishly that knowing what I know now I ought to find the papers easy. I mean that my preparation at school a quarter of a century ago would have been adequate to this examination as it is now. On the other hand, I should have no hope whatever of a creditable degree on the same pretence. The schoolboy of those days was ready for the university of to-day, but his undergraduate contemporary would find a great many of the questions in a modern tripos literally incomprehensible. In other words, while a multitude of ideas have not only been developed in the most highly specialised mathematics during this generation but have permeated mathematical thought to the extent that every educated mathematician knows something of them, the schoolboy is not, as far as I can discover, expected to have a single idea with which his father was not familiar. If this is true, we may

well ask ourselves if it is inevitable or desirable, or if somebody ought to do something about it.

To demonstrate the stagnation of the scholarship examination would be easy, but boring. I should have only to take the papers for a recent year question by question and to point out the parallels set between 1900 and 1910. The one concession I should need is that I must be free to move from one college to another. Thirty years ago there were colleges at which calculus was not expected; to-day there are none. But the questions asked at these colleges now were asked elsewhere then, and taking Cambridge as a whole, there is nothing new. You will find, for example, Laplace's operator in 1901 as well as in 1931.

Oddly enough, my real difficulty would be to prove as conclusively that new ideas have taken root within the university! The various Parts and Schedules of the present tripos do not correspond to steps by which before 1910 a Wrangler acquired Seniority, a Junior Op. acquired a Wooden Spoon, and both acquired fame. Nor is a comparison with the Disorderly Tripos in its tentative early days less unfair. Further, who shall say how much of a syllabus represents knowledge which is common to all the serious candidates? However closely we guard our professional secrets, we admit between ourselves that there are few examinations in which the passmark is 100 per cent. In any non-competitive examination there must be whole sections of subjects which the candidate, did he but know it, can neglect with impunity. Evidence can therefore be only presumptive, but when I find question after question that could not have appeared in my time, I like to believe that this is because the undergraduates who are faced with them learn about things of which we heard little or nothing.

Matrices were so unimportant in the mathematics of those days that only men who read widely or took a pride in an extensive vocabulary ever spoke of them; now a question on ranks and characteristic equations is in place in the very first paper of the Tripos. Not only was vectorial notation heterodox in pure mathematics before the war, but you cannot read such a book as Jeans' *Electricity and Magnetism*, as it appeared in 1908, without being astonished at the pains which the author took to avoid saying simply that one vector was the vector product of two others; now we all speak the language of vectors, in geometry as well as in applied mathematics, and questions on pure vector algebra occur in degree examinations everywhere and not at Aberystwyth alone. To us as undergraduates there was only one integral in the real domain, namely, the Riemann integral as defined by Darboux: the secret of Lebesgue integration was imparted by Hobson to a few postgraduate specialists; it would doubtless be a mistake to infer from the tripos papers that every candidate nowadays is prepared to discriminate between Lebesgue, Perron, and Denjoy, but it is fair to say that everyone who attempts more than the minimum of pure mathematics is aware that Darboux's definition was not the last word. Tensor calculus, the theory

of statistics, relativity, and wave mechanics, all enter into current degree examinations, and although it is only sixty-eight years since Routh identified the curvature and torsion of a twisted curve with components of spin and rendered the artificial use of the corresponding radii for ever obsolete, already last year Cambridge was asking for a formula for κ instead of for the old-fashioned formula for ρ, a reform which Professor Forsyth himself dared not introduce when he was there.

May I take it that you admit the basis of my discourse: the university builds a different mathematical structure, but is content to build it on foundations which have not changed since the beginning of the century.

It may be said that this is inevitable, that the foundations cannot change. Clearly this is the first possibility to face, for if the assertion is true, my subject is exhausted. We are not going to waste the greater part of an hour in deploring the inevitable.

The suggestion is plausible. Certain things must be learned by any mathematician, and it is merely tantalising you to ask you to think of the other things you would teach if you had more time, in days when we all admit the claims of biology and world-history, of other modern languages in addition to French, perhaps also of artistic appreciation and civics, to be elements of a liberal education. The teacher of mathematics cannot expect to be allotted more time, and must think himself fortunate if he is not restricted to less. Let us consider, however, to what claim exactly we are on the verge of committing ourselves. Not merely that the whole time spent on mathematics at school is spent on things which everyone who aspires to be a mathematician must learn: I am sure this is substantially true in many schools. To say that saturation point has been reached is to maintain nothing less than that curriculum and teaching alike are beyond reproach. Is it absolutely certain that the curriculum is perfect, that there is nothing which could be postponed in favour of some subject now acquired at a later stage? Is it quite indisputable that none of the teaching is wasteful, that nowhere would better methods enable us to explain in one hour a principle over which we have got into the habit of spending two? I do not say that you will not answer these questions with a confident affirmative; I am observing that the plausibility has disappeared on analysis: the teacher is on the defensive.

In parenthesis, I admit that the distinction between method and material which has enabled me to put two rhetorical questions instead of one, though often convenient for purposes even more respectable than this, is not really valid. Hardly ever should we cover precisely the same ground by alternative routes. If we prove the addition theorems for the cosine and sine once for all by projection, there is no reason why our students should ever know the various ingenious geometrical proofs applicable only to signless acute angles, or should ever bother with the tiresome verifications that extend the formulae to the unrestricted angle. If there is no mysterious rule of

the game to forbid us from establishing the sine and cosine expansions by means of simple inequalities in integration, there need be no premature efforts to grapple with Tannery's theorem.

To resume. If we agree that it is conceivable that room could be made for new ideas by omissions or condensations, it might seem that we should begin at once to consider in detail the possibilities of change. But we must not assume too readily that omission and condensation is a proper price to pay for improvement. What of the argument, with which excellent teachers have flirted, that the individual should recapitulate the history of the race, and that, just as the physical embryo lives through evolutionary phases which the adult creature has outgrown, so in the normal mental development of the mathematician, ideas should come into his brain in the vague and unsatisfactory forms in which they came into the thought of the race, to be criticised as if for the first time and to be rejected or clarified afresh?

I do not subscribe for a moment to this theory. There is nothing intrinsically probable about it. Daily life is full of simplifications which not only supersede but obliterate clumsy methods which they replace. In the early tube stations in London, lifts had only one gate, and since the first passenger to reach a lift was reluctant to be the last to leave it, a tight jam over one-third of the floor space was compatible with a half-empty lift. Nobody maintains that when tube travel was revolutionised by the installation of lifts with two gates, a few lifts of the archaic pattern should have been kept in exemplary use ; still less that no passenger should be allowed to use an escalator who could not produce a certificate of personal experience of the inconvenience of the one-gated lift.

The biological analogy is utterly false. The race lived for æons on the water's edge, for millenia in the branches of trees ; the infant is not rocked in the cradle of the deep or in that other cradle in the tree top in the belief that only so can he be fitted for life on the firm ground. And our vestigial organs, that survive in fact but not in function, far from providing models which we are glad to copy in mental affairs, are such incipient nuisances that we pay in time and agony and hard cash for their removal from our systems.

Perhaps the principle of recapitulation, which after all is the inevitable perversion that the heuristic principle suffers in all but the best hands, can be presented more fairly in a different metaphor. The individual learner must follow the same path as the race, but he may be guided rapidly where the race has stumbled slowly and painfully along : the teacher does not abdicate. This sounds well enough, a compromise in which evolution is recognised with only a reasonable sacrifice of time. Also every teacher tender or otherwise is at home in the part of Father O'Flynn :

> "Checking the crazy ones, coaxin' the aisy ones,
> Lifting the lazy ones on wid the stick".

But this is just the trouble. On these lines the limit of condensation has probably been attained, and if we object on principle to

omission, it does follow that we must abandon hope of modifying the curriculum.

To refute the principle that leads to this conclusion, I rely less on argument than on illustration. This is not illogical. To overthrow a universal proposition only one exception is needed, and when one has been found, other cases stand or fall on their merits. It is easy to find in mathematics methods and ideas which are outworn; valuable in their time, their only legitimate use now is as a quarry for technical exercises. I dare not take examples within the range of school mathematics, since I have neither the knowledge of psychology nor the experience of teaching to meet a challenge in any particular instance, but if I show you what I mean at the undergraduate level, you will hardly urge that a natural boundary separates the school from the university.

In the earliest applications of analysis to the geometry of curved surfaces, it was taken for granted that a surface was represented by an equation expressing one coordinate z implicitly if not explicitly as a function of the other coordinates x, y. Monge, writing in the month of thermidor in the year 9, takes his surface in the form $M=0$, where M is a function of x, y, and z, but he makes his deductions from the equation which Cauchy and Dupin afterwards wrote in the form $dz=p\,dx+q\,dy$ and called the differential equation of the surface. Since the very conception of coordinates developed as we might say piecemeal, z measuring an offset from a plane in which some facility with x and y had been acquired, from what other representation could the first application of analysis to the geometry of a surface have been expected to proceed? But from the moment when Gauss in 1827 remarked that a surface is essentially the aggregate of positions of a point whose coordinates depend on two independent parameters, the unsymmetrical equation $z=f(x, y)$ and Monge's unsymmetrical differential equation could do nothing but obscure the fundamental ideas of the subject, however useful they might be in special problems. What is more, from the moment when Hamilton first saw a vector as a geometrical entity and began to look for the numbers and the vectors associated intrinsically with a given set of vectors, Gauss' functions which most of us know best as E, F, G and L, M, N could be seen to have no dependence on the cartesian coordinates from which Gauss constructed them, but to emerge inevitably from the velocities and accelerations of the current point with respect to the variables of reference on the surface. The student of mathematics to-day does not approach the theory of surfaces until he is long past the stage of thinking of z as an offset from the xy plane, and until he is well acquainted with vectors and their products; accordingly, none of us regard the equation $z=f(x, y)$ as a natural or convenient basis for the theory, and more and more of us emphasise the velocities of the current point rather than algebraic combinations of derivatives of the coordinates. That is to say, we do not follow Monge's pioneer path, and we reach an advanced point of Gauss' track by a short cut which he could not

take. We shall of course find formulae for the surface $z=f(x, y)$ by way of exercise, and we shall point out that if the current point is identified by coordinates in a trirectangular frame, the magnitude E, being the square of the strength of the velocity whose components are the derivatives of x, y, z with respect to u, is calculable as the sum of the squares of these derivatives. But this is quite different from investigating a surface in general by means of the unsymmetrical equation and from defining E in terms of x_1, y_1, z_1.

A second example comes from the history of the concept of an irrational number. The irrational cannot be defined as the limit of a sequence of rationals. When the only numbers known are rationals, all that can be said of such a sequence as

$$(\tfrac{3}{2})^2,\ (\tfrac{4}{3})^3,\ (\tfrac{5}{4})^4,\ (\tfrac{6}{5})^5,\ \dots,$$

when it has been proved that it has no rational limit, is that it does not possess a limit. To say that because it has no rational limit, therefore a new kind of number exists, is nonsense. In a passage that has often been quoted, Russell remarks: "The method of 'postulating' what we want has many advantages; they are the same as the advantages of theft over honest toil. Let us leave them to others". Well, we have been told that property is theft, and I suspect that the future biographer will use this passage to prove that the Russell of 1903 was no communist. Fortunately, we have not to reproach the thief on the high and risky moral ground that he takes what he wants without paying for it, but to jeer at him for not perceiving that what he takes is not in the least what he wants: the postulated limit is Dead Sea Fruit, Fairy Gold, a Prince Rupert's Drop that shivers to powder at a touch.

Cantor—the philosopher, not the historian—long before Russell and no less clearly, perceived that the irrational cannot be brought into existence as a limit of rationals. But, said Cantor, although we cannot identify a limit of such a sequence as

$$(\tfrac{3}{2})^2,\ (\tfrac{4}{3})^3,\ (\tfrac{5}{4})^4,\ (\tfrac{6}{5})^5,\ \dots$$

in the arithmetic of rational numbers, we can say perfectly well in terms of rationals alone what we mean by describing the sequence as convergent. Further, if we compare this sequence with the three sequences

$$\frac{7}{5},\ \frac{41}{29},\ \frac{239}{169},\ \frac{1373}{985},\ \dots,$$

$$1+1,\ 1+1+\frac{1}{2!},\ 1+1+\frac{1}{2!}+\frac{1}{3!},\ 1+1+\frac{1}{2!}+\frac{1}{3!}+\frac{1}{4!},\ \dots,$$

$$\frac{1}{4}\left(\frac{3}{2}\right)^2,\ \frac{1}{6}\left(\frac{3.5}{2.4}\right)^2,\ \frac{1}{8}\left(\frac{3.5.7}{2.4.6}\right)^2,\ \frac{1}{10}\left(\frac{3.5.7.9}{2.4.6.8}\right)^2,\ \dots,$$

we soon find that it has an intimate relation to the second which it does not have to the other two. For the sake of brevity, while avoiding the associations of any technical word, we might say for a moment that the original sequence and the second of these three

are intimate with each other. We can prove easily that sequences which are intimate with the same sequence are intimate with each other—which perhaps implies that the adjective is not at all a happy choice!—and then, as in the case of any symmetrical transitive relation, we transfer our attention from the individual sequence to the class of mutually intimate sequences. Actually what we say is not that two sequences are intimate, but that they tend to the same limit, and if we care to use this phrase while protesting that we do not mean to suggest that there is any such thing as a limit to which they tend, this is a mere foible of language of no logical significance whatever, like the picturesque habit of saying that parallel lines meet at infinity while denying that there is any infinity where they can meet. In this way Cantor establishes not indeed a mathematics in which real numbers are said to exist, but a mathematics in which all sorts of unsophisticated statements about real numbers are interpreted to acquire a meaning. I have no intention of talking about either the technical details or the philosophical difficulties of this theory of classes of sequences. On the contrary, it is precisely because, now that we have a simpler theory of real numbers, this is an example of a theory on which no one but a historian or a philosopher should spend time, that I have spoken of it.

If I have brought you with me, we are agreed that it is neither absurd nor improper to suggest that the range of school mathematics could be changed, and that since progress certainly is accelerated in some stages of mathematical education by the complete neglect of methods and theories which have played important parts historically, an inelastic time-table is not in itself an insuperable barrier. It follows, surely, that it is as much the teacher's duty today to ask whether he is in danger of stagnation as it was when our Association was founded sixty-four years ago.

It is not necessary for me to insist from this Chair that Improvements in Teaching have been effected. Verbal perfection in uncomprehended theorems of Euclid has been replaced by an understanding of such general relations as symmetry and similarity, and there is solid geometry at the very beginning of the school course instead of only at the very end. The time that was once devoted to the manipulation of complicated fractions is now spent on graphs. Elementary calculus has supplanted elaborate trigonometry. Can we suppose that this process is at an end? It has often been remarked that the profoundest effect of the Copernican hypothesis was not that it displaced the earth from the material centre of the universe but that it displaced man from the philosophical centre: it is not possible for the most bourgeois of us to believe that the whole gorgeous pageant of space and time was designed with special reference to the inhabitants of a middle-class attendant on a middle-class star. But have you noticed that indeed the conviction of our unimportance has taken command of our cosmological speculations? When Russell Wallace put forward a theory of man's place in the universe which restored the sun to the centre of the starry realm and found in the

earth the sole abode of life, the theory was suspect for that very reason. Man may be essentially an ape with a swelled head, but he is not so swollen headed as really to believe that the heavens declare *his* glory. We can endure the knowledge that we are rare, and we do not resent Jeans' estimate that we may well be at this instant the most ignorant cosmogonists in space, but we shrink from the pretence that there never was and never will be anything like us. In much the same way, whether or not we can avoid Shaw's conclusion that improvement must presently cease unless life can be prolonged, we find it hard to believe that we ourselves are at the critical moment. If we ask, not " Can we suppose that the process of improvement in teaching is at an end ? " but " Can we suppose that the process has just come to an end ? " all our instincts are against an affirmative answer.

To be frank, is there any doubt on this matter ? Is not each one of us itching to make changes, small or large, and restive because he thinks he dares not sacrifice a shred of the common curriculum in the interests of his own pet reform ? How many of you, for example, share my conviction that the time is overripe for reassessment of the conceptions of sequences and bounds in analysis ? Most of us, I think, were brought up to believe that the exponential theorem is essential to the rigorous definition of a power : for irrational values of x, a^x was defined for us as $\exp(x \log a)$. This was always something of a shock. If the definitions of $3^{1\cdot 4}$, $3^{1\cdot 41}$, $3^{1\cdot 414}$, ... are only as complicated as any reasonable schoolboy would expect, why this incursion of the particular number e into the very definition of $3^{\sqrt{2}}$? The cause can be nothing but the exaggerated deference paid to the sequence, whereby a number is held to be better defined as the limit of some one sequence than in any other way. If a and x cannot be expressed except by means of sequences $a_1, a_2, a_3, \ldots$ and $x_1, x_2, x_3, \ldots$, and if the individual power

$$a_m^{\;x_n}$$

also requires some sequence $(y^{mn})_1, (y^{mn})_2, (y^{mn})_3, \ldots$ to give it meaning, how shall we arrive at a definition of a^x ? We must first have a correlation of m with n in order to select from the doubly infinite set of powers of the form

$$a_m^{\;x_n}$$

one sequence

$$(a_{m_1})^{x_{n_1}},\ (a_{m_2})^{x_{n_2}},\ (a_{m_3})^{x_{n_3}},\ \ldots ;$$

we must then select from the sequence

$$(y^{m_s n_s})_1,\ (y^{m_s n_s})_2,\ (y^{m_s n_s})_3,\ \ldots$$

one member $y^{(s)}$; subject to satisfactory rules of correlation and selection, the sequence $y^{(1)}, y^{(2)}, y^{(3)}, \ldots$ will specify a real number ; lastly we shall have to show that with a further but not unnatural tightening of the rules, the real number to which we are led is unique. None of the steps in this elaborate and tedious piece of work can be expected to have any intrinsic interest or any bearing on

other problems. It is little wonder that the alternative of waiting until a simple definition can be given was overwhelmingly attractive.

But if we are accustomed to thinking in terms of aggregates and bounds, and if our conception of a real number is not dependent on sequences, how different it all appears. For the signless real number a and the rational p/q, the real number $a^{p/q}$ is just as specific and as independent of arbitrary sequences as the number a itself, and a^{ξ} is a bound of the simplest kind. If we prove that for any given value of the whole number n there must lie between any two signless rationals a number which is the exact nth power of some rational, the whole theory of a^{ξ}, depending as it does on the fundamental identities $a^{\xi}\beta^{\xi}=(a\beta)^{\xi}$ and $a^{\xi}a^{\eta}=a^{\xi+\eta}$, follows at once. If this is our method, we must of course establish that nth powers are dense among the rationals without assuming the continuity of a^n as a function of a, but this requires nothing more than an elementary case of the inequality $x^n-y^n \leqslant n(x-y)x^{n-1}$, an inequality which, far from being a mere lemma to this theory, is of the utmost importance throughout analysis.

To give further examples of changes I should like to see would be somewhat to abuse my opportunity. You will have noticed that I said just now that you dare not make individually the changes on which your minds are set, and this raises the question whether I am not talking to the wrong audience. The university, it may be said, calls the old tune, and the school can do no more than train the boy to dance to it as gracefully as may be. Much as I should like to hear new themes creeping into the tune, I am not disposed to blame the universities. The primary purpose of a scholarship examination is to discover good scholars, not good teachers, and papers on which a boy who had learned all that is in the more popular textbooks was not certain to be rewarded would defeat its own object; a genius is not the better for being self-taught, but a self-taught genius is worth many well-drilled mediocrities. But this does not mean that good teaching does not pay, in the crudest sense, nor must we forget that preparation for university scholarships in mathematics is only a small fraction of a mathematical schoolmaster's responsibility. Let me expand each of these observations in turn.

Genuine understanding and breadth of ideas, far from being an alternative to technical skill, are almost always its surest guarantee. Some of you remember the days when the most elementary calculus was a mystery, initiation into which was postponed as long as was humanly possible. I know I borrowed a calculus book myself and read it long before any teacher thought I was of age, but I don't suppose I was any younger than the average beginner of to-day. In the examinations of one great university till quite recently pure mathematics for a degree excluded calculus on the letters side, though not on the science side. The syllabus in applied mathematics on the letters side was therefore compiled to be congenial to the artistic temperament as so conceived; this involved the sacrifice of so much straightforward mechanics that was included as a matter of

course on the other side, that by way of redressing the quantitative balance the artist had to learn astronomy but the scientist might remain ignorant of the subject. This was an extreme case, but no one who knows anything of the tricks and stratagems by which statics and dynamics were prosecuted without calculus will dispute that the stupidest students reach the highest level of which they are capable if they are given a rudimentary general notion of rates of change and taught the elementary integrals*. On the same principle, Hobson, after listening to a paper in which the resources of manipulation in pure algebra had been strained to the utmost to provide direct verification of some well-known identities in hypergeometric functions which can be proved in a few lines by the methods of the theory of functions, made the private comment: "Very ingenious and painstaking, but I could never see that a sack-race is athletics." And most of us can recall to this day the relief with which we learned to prove the fundamental theorem of algebra, that an algebraic equation has a root, in two lines by Liouville's application of Cauchy's theorem instead of in two pages by ill-digested arguments on the change of angle of a function of a complex variable or on the continuity of a polynomial in two real variables.

I do not wish to dispute that there is another side to this question. In my first term at Cambridge I showed off by using Lagrange's equations on a simple problem, and all the praise I had from Herman was the well-deserved snub: "Why are you using a steam-hammer to crack your nuts?" The right answer, which he probably knew if I did not, was that my hands were too weak to use nut-crackers: I was using Lagrange's equations automatically because I could not apply the first principles of dynamics intelligently. This is a definite danger. Nor must we overlook the cultivation of a sense of the fitness of things, on which Professor Hardy often lays stress; a theorem in algebra, he tells us, should have at any rate one standard proof by algebraic methods, and one wonders if he can ever forgive Hilbert for proving the truth of Waring's conjecture by means of a multiple integral. I doubt whether this aesthetic principle extends over the whole of mathematics. In fact the geometer seems here to part company with the analyst, for nothing makes a geometer happier than to recognise in a complicated theorem of plane geometry a mere cross-section, so to speak, of a straightforward configuration in space of several dimensions, and none of us think the worse of Clifford because it was by means of the parabola of the nth class that he established the theorem on chains of circles which unites his name with Miquel's.

A broadening of outlook, by promoting technical skill, helps us to train competent workmen, and that is all we aspire to do with most of our special students. What of the boy with scientific bent who is not reading for a scholarship in mathematics? And what of the vast majority, boys to whom mathematics is just part of a general

* Compare the apposite quotation from Todhunter in the Gleaning which follows this Address.

education, and on whom the scope of our scholarship papers has no direct effect ?

Well, in the reforms of the past the vast majority has gained even more obviously than the skilled minority. Informal generality in geometry, graphs in algebra, and the elements of the calculus, to repeat the examples I have used already, benefited the born mathematician, certainly, but to the born non-mathematician they brought purpose and interest into dead hours.

In relation to the developing scientist there is a different argument. On the whole, what matters is not that he should learn to do simple sums for himself, but that he should gain as wide a knowledge as possible of the kinds of problems with which mathematicians do deal. I have never forgotten the surprise with which I once heard a lecturer in a textile department expounding to an audience of mathematicians his discoveries of properties of a pattern which repeats itself in two distinct directions : how there must be other directions of repetition, how there is a minimum repeated area which has different shapes according to the pair of directions which is taken as primitive, and so on ; in short, the elements of the theory of double periodicity, with which his hearers had been perfectly familiar for years. Einstein's success in developing the general theory of relativity was due in no small degree to the fact that he knew that the mathematical foundation he required was actually in existence. Among the changes which have taken place in pure mathematics in the last half-century is the subordination of the exact formula, the perfect identity. To the intelligent layman, typical mathematical questions are : "What is the equation of this curve ?" "What is the formula for that function ?" He takes for granted that mathematics in any serious sense is necessarily quantitative and exact, and unless he can see for himself that his problems can be formulated as quantitative and exact, he thinks they are not fit for mathematical study. That we can go further in topology or enumeration than common sense will take him in half-an-hour never crosses his mind. That there is a splendid calculus of groups and patterns he does not know. If you tell him that the last thing a mathematician wants to do with an equation, algebraic, functional, or differential, is to solve it, he wo'n't have the least idea what you mean. And if you tell him that when a mathematician wants a numerical answer to a numerical question he can always get it, he will not believe you. Is he to blame, or is his view of mathematics the only view he has been allowed to see ? Could he but understand that a vast amount of analysis to-day is concerned with the estimation of the sizes and relative importance of terms that are not evaluated, and with the distribution of zeros of functions which satisfy equations that are not solved, could he realise that the mathematician knows that a problem is by no means solved when a formula is found, how much readier he would be to bring his questions to be restated in that formal and abstract language in which, and not in measurement, the essence of a mathematical problem resides.

A word about the real mathematician. Our encouragement in the dullest hours is the hope that presently we shall see in front of us again a pupil whose face lights up with almost a lover's excitement at what we have to teach. What of our responsibility when that hope is fulfilled? If we can give such a learner a groundwork designed in relation not to branches of mathematics which became barren twenty years ago, but to the vital growths in which he will soon be participating, we shall be making our own modest and anonymous contribution to the advancement of knowledge.

The burden of my plea this afternoon is that changes in emphasis in creative mathematics, which have now a direct influence on teaching at the university, ought to have a greater and a far more rapid influence on teaching at the school than they seem to have. If you ask me how this is to be brought about, I have no answer ready. "Si jeunesse savait, si vieillesse pouvait!" runs the old song: "If youth but knew, if age but had the power!" Is it fanciful to insist that we are confronted with a perversion of this plaint? The young teacher has the knowledge of what is important, the old teacher has the experience of what is practicable and the influence to effect the changes he desires. A few of my contemporaries are beginning to find their textbooks accepted as authoritative: here and there a book that established itself thirty years ago is being replaced. This is all wrong. The clock is a quarter of a century slow. My friends will not misunderstand me if I say that as writers for schools most of us ought to have had our day and to be, technically speaking, pulped. Twenty years ago we did know what were the best current methods of presentation, where emphasis had to be placed to serve most efficiently the needs of those who were soon to be undergraduate students of mathematics. How many of us who are engaged in teaching rather than in research can make the same boast to-day? Only a month ago Dr. Ramsey reminded us in the *Gazette* that no one who is not in residence can be expected to know that it is twenty-five years since the cylindroid was mentioned in a Cambridge lecture-room.

If I was content to give you the impression that the problem is to give knowledge to the old and power to the young, how I should now beat the big drum of propaganda! Here is the Association for the dissemination of knowledge, and the editor of the *Gazette* can see to it that if our youngest members do not write the books they shall write the reviews. But can we not see rather an opportunity to replace the rivalry of the generations by cooperation, thus performing in our own sphere the miracle which is the hope of the world?

And to what end? Need I explain now why I have given my discourse the title of a work of genius which begins as a pseudo-scientific joke and culminates as the most magnificent of modern allegories? The basis of the story to which I refer is the assertion that growth is spasmodic: short bursts of rapid development alternate with long periods during which the organism is almost stationary, and the difference between one growth and another is less

in the rate at which change takes place when it is taking place at all than in the length of the still intervals. Find nourishment for which the periods of accommodation are not required, and you have the food of the gods, on which man can thrive to a stature beside which we cannot without magnanimity bear to measure our puny selves. To ask in earnest whether the biological pretence has any foundation in fact would be contrary to the spirit in which such a story should be read. But the hypothesis is a very true picture of the more elementary stages of mathematical teaching; each advance is followed by some score of years in which almost all that happens is that the backward schools and the popular textbooks are creeping up to the leaders, while here and there an isolated adventurer fails to attract followers. Can we envisage such responsiveness everywhere to the ferment of current ideas that the growth curve of our mathematics, instead of resembling the section of a worn shallow flight of steps, loses the horizontal treads and rises steadily and more steeply, until in our more cheerful dreams we, like the biochemist of Wells' imagination, see it in the form

E. H. Neville.

GLEANINGS FAR AND NEAR.

995. Trembley's methods are laborious, and like many other attempts to bring high mathematical investigations into more elementary forms, would probably cost a student more trouble than if he were to set himself to enlarge his mathematical knowledge, and then study the original methods.—I. Todhunter, *Hist. of the Math. Theory of Probability*, § 767, p. 415. [Per Dr. G. J. Lidstone.]

996. The length of the most common words is a serious obstacle, especially in teaching; and no body of educated men ever had the sense of the people at large, *quem penes arbitrium* merely because they choose that it shall be so .... That nothing shorter than "the differential coefficient of y with respect to x", sixteen syllables of sound and forty-three letters of writing, can be found to express the ultimate element of the differential calculus, is a misfortune and a discredit. And more especially when it is remembered that this conglomerate of letters does not express the modern meaning of the symbol .... The reform which I should propose, if it were possible to create a discussion, would consist in expressing $dy : dx$ as "the rate of y to x" and "the x rate of y", in abbreviation of "the ratio of the rate of variation of y to that of x" .... It is very much to be regretted that the notion of fluxions disappeared with the notation.—A. De Morgan, *Camb. Phil. Trans.*, vol. x (1864), pp. 425-6. [Per Dr. G. J. Lidstone.]

SOME ASPECTS OF THE DEVELOPMENT OF MODERN STATISTICAL METHOD.*

By J. O. Irwin.

There are two main roots to the tree of modern statistical method: the longer one takes us back to the mathematical investigations into games of chance of the seventeenth and eighteenth century mathematicians, the other starts with the statistical researches of Quetelet and Galton.

Of the former we are told by Poisson "that a problem, relative to games of chance, proposed to an austere Jansenist by a man of the world, was the origin of the calculus of probabilities". We find from Todhunter's *History of the Theory of Probability* that the problem was as follows: "Two players play a game of chance in each stage of which a point may be won or lost. The players, at a certain stage of the game, want each a given number of points in order to win; if they separate without playing out the game, how should the stakes be divided between them?" The problem was proposed by the Chevalier de Méré, a noted gamester, to the mathematician Pascal about 1650, and an extensive literature exists on it. Pascal and Fermat exchanged a number of letters on the subject, indeed it was the principal subject discussed by them; but, says Todhunter, "It was certainly not exhausted by them. For they confined themselves to the case in which the players are supposed to possess equal skill, and their methods would have been extremely laborious if applied to any examples except those of the most simple kind". The problem was discussed later on by Huygens and by James Bernoulli, but the first complete solution, allowing as it does for unequal skill of the players, was given by Montmort in his *Essai d'Analyse sur les Jeux de Hazards*, published in 1708, where, says Todhunter, "With the courage of Columbus he revealed a new world to mathematicians".

Another of the problems which exercised the mathematicians of those days is the famous problem of "Duration of play". This is the problem: A has m counters and B has n counters; and their chances of winning in a single game are as a to b; the loser in each game is to give a counter to his adversary; required the chance for each player of winning all the counters of his adversary, and also the probable duration of the game. A particular example of the problem was first stated by Huygens: "A and B take each twelve counters and play with three dice on this condition, that if eleven is thrown A gives a counter to B and if fourteen is thrown B gives a counter to A; and he wins the game who first obtains all the counters". Find A's and B's chance of winning the game.† The problem of

* Chairman's address to the Study Group of the Royal Statistical Society, 1934-35.

† A's chance is to B's as 244,140,625 to 282,429,536,481.

finding the two players' chances of winning the games is easily solved by means of the calculus of finite differences. It was first solved by James Bernoulli in his *Ars Conjectandi,* which we shall have occasion to refer to again ; but solutions were published earlier by Montmort and Nicolas Bernoulli in 1708 and by De Moivre in his *De Mensura Sortis,* a memoir which appeared in the *Phil. Trans. Roy. Soc.* for 1711. James Bernoulli died in 1705, but the *Ars Conjectandi* was not published until 1713.

The problem of finding the probable duration of play is a much more difficult one ; it was discussed by Montmort and very thoroughly treated by De Moivre, who solved it completely for the case when both players start with an equal amount of money, and for the case when one of them has unlimited capital. De Moivre's solution appeared in the first edition of his *Doctrine of Chances,* published in 1718. The case when the players start with unequal amounts of money was solved by Laplace. The problem was also discussed by Simpson (1740), Lagrange (1777), Waring (1792), and by Laplace, who in his *Théorie Analytique des Probabilités* (published in 1812) gave a comprehensive treatment which, however, owed much to De Moivre and Lagrange. The most recent treatment of the problem is that of E. C. Fieller in *Biometrika* for 1930-1, who attaches a historical note in which he expresses the opinion that the most elegant treatment of the subject is that of Robert Leslie Ellis in the *Cambridge Mathematical Journal* for 1844. A problem which has interested mathematicians for more than 200 years cannot be without fascination and interest.

These two problems must suffice as examples of the enormous literature on the theory of games of chance. This literature is not without its humour ; for instance, Montmort worked out the chance of winning at a game called "*Le Jeu des Noyaux*", which he says the Baron de la Hontan has found to be in use among the savages of Canada. I quote Todhunter : "Montmort says that the problem was proposed to him by a lady who gave him almost instantly a correct solution of it ; but he proceeds very rudely to depreciate the lady's solution by insinuating that it was only correct by accident ; for her method was restricted to the case in which there were only two faces on each of the dice ; Montmort then proposes a similar problem in which each of the dice has *four* faces. Montmort should have recorded the name of the only lady who has contributed to the Theory of Probability".

At first sight we might think that a great deal of energy was wasted here on trivial problems. But this is not so ; the methods developed in games of chance led to the idea of mathematical expectation which lies at the basis of all actuarial calculations, and in another direction led to the theory of frequency distributions, especially those which occur in the sampling problems which are the basis of modern statistical theory. Besides, it was not long before these early inventors of the theory of probability turned their attention from games of chance to more important questions. The

first time this happened was, as far as I know, in James Bernoulli's *Ars Conjectandi*. The first part is a reprint with a commentary of Huygens' treatise *De Ratiociniis in Ludo Aleae,* the second contains the doctrine of permutations and combinations, the third part consists of problems illustrating the previous theory, but it is the fourth part which is the most interesting ; it is entitled *Pars Quarta, tradens usum et applicationem pracedentis Doctrinae in Civilibus, Moralibus et Oeconomicis*. The most interesting thing which it contains is a demonstration of what is known as Bernoulli's theorem. This can be enunciated as follows : " If the probability of an event is p, then if we take a fraction ϵ as small as we please, we can make the probability that the proportion of successes in n trials differs from p by more than ϵ as small as we please, by taking n large enough ". In other words, the frequency of occurrence in large samples will tend to be p. Bernoulli demonstrated his theorem with a remarkable degree of rigour, considering the time at which he wrote. Of course it is the inverse of the theorem, which, stated in modern language, concerns the accuracy with which we can predict from a sample the proportion bearing a certain mark in the population from which the sample is drawn, that is most interesting to us and at the same time presents the most difficulties.

The idea of mathematical expectation is, as we have said, closely connected with the idea of a life table, and we must digress from our mathematicians interested in probability to notice two most important contributions to the methods of vital statistics. The first is John Graunt's *Natural and Political Observations on the Bills of Mortality*, an early edition of which those of you who were at the Guildhall at the time of the Royal Statistical Society's centenary may have seen. It was published in 1662, and Graunt constructed a table showing the ages and causes of death in the city of London from 1628-37. An amusing passage occurs in Todhunter in this connection ; it is quoted from Lubbock and Drinkwater :

" They were first intended to make known the progress of the plague ; and it was not until 1662 that Captain Graunt, a most acute and intelligent man, conceived the idea of rendering them subservient to the ulterior objects of determining the population and growth of the metropolis ; as before his time, to use his own words, ' most of them who constantly took in the weekly bills of mortality, made little or no use of them than so as they might take the same as a text to talk upon in the next company ; and withal, in the plague time, how the sickness increased or decreased, that so the rich might guess of the necessity of their removal, and tradesmen might conjecture what doings they were like to have in their respective dealings '. Graunt was careful to publish with his deductions the actual returns from which they were obtained, comparing himself, when so doing, to ' a silly schoolboy coming to say his lesson to the world (that peevish and tetchie master) who brings a bundle of rods, wherewith to be whipped for every mistake he has committed '. Many subsequent writers have betrayed more fear

of the punishment they might be liable to on making similar disclosures, and have kept entirely out of sight the sources of their conclusions. The immunity they have thus purchased from contradiction could not be obtained but at the expense of confidence in their results." The first life-table was constructed by Halley in a memoir published in the *Phil. Trans. Roy. Soc.* for 1693. It is entitled *An estimate of the degrees of the Mortality of Mankind, drawn from curious Tables of the Births and Funerals at the City of Breslau; with an attempt to ascertain the Price of Annuities upon Lives.* Halley obtained his life-table by totalling the deaths at ages, a procedure only strictly correct for a stationary population.

Most important contributions to the theory of life-annuities were made by De Moivre, who came to England as a boy of eighteen in 1685 and lived here until his death in 1754. His contributions to the theory of probability Todhunter considers second only to those of Laplace.

We cannot discuss his work at length, but there is one point in it to which we *must* refer. He discovered, and as far as we know was the first to do so, the normal frequency distribution. I quote Professor Karl Pearson here: "The matter is a very singular one historically. De Moivre published in 1730 his *Miscellanea Analytica*, still a mine of hardly fully explored wealth. Many copies of this work have attached to them a *Supplementum* with separate pagination, ending in a Table of 14 figure logarithms of factorials from 10! to 900! by differences of 10. But only a *very few* copies have a second supplement, also with separate pagination (pp. 1-7) and dated November 12th, 1733. This second supplement could only be added to copies sold three years after the issue of the original book, and this accounts for its rarity. Dr. Todhunter in writing his *History of the Theory of Probability* appears to have used the 1730 issue of the *Miscellanea Analytica* and so never come across the Supplement". The supplement is entitled—I translate the Latin—*Approximation to the Sum of the Terms of the Binomial* $(a+b)^n$ *expanded in a series.* It is only necessary to say here that by a use of Stirling's theorem that anticipated Stirling he obtained the normal curve as an approximation to the binomial series, and discussed its probability integral in a very modern way. "Nor", says Professor Pearson, "did Todhunter in the least appreciate the immense range of application which De Moivre himself foresaw for his problem; for him it was a theological problem, he was determining the frequency of irregularities from the Original Design of the Deity. Without grasping this side of the matter, it is impossible to understand the history of statistics from De Moivre through Derham and Süssmilch to Quetelet, culminating in the modern principle of the stability of statistical ratios".

Among the early applications of the theory of probability to vital statistics was the work of Daniel Bernoulli (in 1766), and of D'Alembert a year or two later, on determining the mortality there would be if smallpox were eliminated, given the survivors at ages in the

whole population and the deaths at ages from smallpox. Daniel Bernoulli assumed that smallpox attacked every year one in n of those not previously attacked and that one died out of every m attacked, and by taking $m = n = 8$ was able to obtain numerical results. D'Alembert was content to leave his result in the form of an integral. D'Alembert's formula was used as late as 1930 by Miss M. N. Karn to construct life-tables free from the influence of particular disease (such as cancer or tuberculosis); though, as a matter of fact, the problem can be solved without integral calculus by a simple formula due to Dublin and Lotka. Trembley's results, published in 1799, can also be established by the same principle.

We must now turn to the early history of the "Theory of Errors of Observation". This is by far the most important technique we have to discuss in the history of the subject, because the mathematical methods evolved are applicable with little change to the two main problems of statistical method, the problem of estimating from a sample certain quantities in the population from which the sample is drawn, and the problem of assessing the accuracy of the estimations so made.

The first people who seem to have studied the theory of errors of observation were Thomas Simpson, Professor of Mathematics at the Royal Military Academy, Woolwich, Lagrange and Daniel Bernoulli.

Simpson's work was published in 1757 in his *Miscellaneous Tracts on some curious and very interesting subjects in Mechanics, Physical-Astronomy and Speculative Mathematics.* It is entitled "An attempt to shew the Advantage arising by taking the Mean of a Number of Observations in Practical Astronomy". Lagrange's work is in the *Miscellanea Taurinensia* (Lagrange was born in Turin) for the years 1770-73. Both Simpson and Lagrange give the exact form of the frequency distribution of the mean in sampling, for samples of any size, on the assumption that the frequency curve of the observations in the population sampled is an isosceles triangle. This was a very remarkable result for so early a date, and was obtained by purely algebraical methods. But Lagrange had arrived at a very modern method for finding the frequency distribution of a mean; what we now call the method of moment generating functions. In 1926, in a paper in *Biometrika,* I gave a general formula, for the frequency distribution of the mean in sampling from any population, for any size of sample. The result is in the form of a single integral, and I showed how it might be applied to certain particular cases. Among these cases was that when all values in the population are equally probable—the so-called rectangular frequency distribution; by another route Philip Hall obtained a result agreeing with mine for that case. It is only in the last few weeks that I have found out that Lagrange gives the same result about 160 years earlier. Lagrange did not, I think, obtain the integral form of the general solution, but his work shows that he was fully acquainted with the method by which we obtained it.

Daniel Bernoulli's contribution, published in the *Transactions of*

the St. Petersburg Academy in 1778, suggested that, since small errors were certainly more probable than large ones, a semicircle should be taken as the curve of distribution of errors; this paper is followed by some remarks by the distinguished mathematician Euler.

But the modern form of the theory of errors of observation is due mainly to Laplace and Gauss.

Laplace was born in 1749 and died in 1827. He published some memoirs on the theory of probability at a very early age, but his *Théorie Analytique des Probabilités*, perhaps one of the greatest mathematical works ever written, was not published until 1812. Laplace was fully aware of the enormous field in the physical and social sciences afforded by the theory of probability, in fact, by what we should now call statistical methods; and he did much, considering the date at which he wrote, to explore that field. In Todhunter's account, applications to astronomy and vital statistics seem to strike one most, apart from games of chance; but I have noted one meteorological example: an attempt to estimate whether the mean temperature in the morning differs from that in the afternoon. As an example of his work in vital statistics we may quote the problem of estimating the population of France from the known number of births, by taking a sample in which one knows both the population and the births, subsequently estimating the accuracy of the result. He concluded that we ought to take a district containing not less than a million people in order to estimate the population of France sufficiently accurately.

Towards the end of his introduction Laplace says: "It is remarkable that a science which began with the consideration of games should have raised itself to the most important objects of human knowledge". At the end of it he says: "One sees by this essay that the theory of probabilities is basically nothing but common sense reduced to calculation; it enables us to assess with exactitude, what sound minds feel by a sort of instinct, without being able often to account therefor. If one considers the analytical methods to which this theory has given birth, the truth of the principles on which it is based, the fine and delicate logic which is demanded by their employment in the solution of problems, the matters of public importance which are dependent on them, and the extension which it has received and which it can yet receive, by its application to the most important questions of natural philosophy and moral science; if one observes finally, that in matters which cannot be submitted to exact calculation it gives the most sure viewpoints which can guide our judgments and that it teaches us to avoid illusions which often mislead; one sees that there is no science more worthy of our meditations, and that it would be most useful to introduce it into our system of public instruction". There is much in these words which would still be applicable to-day.

After this digression let us ask what Laplace achieved in the study of the theory of errors of observation. Briefly, he showed that if an error were the resultant of a large number of chance com-

ponents, each with its own frequency distribution in which (though this is not essential) deviations in excess or defect were equally likely, then the frequency distribution of errors would follow what we now call the normal frequency distribution. He then deduced the method of least squares as a consequence, showing that this would be the best method of estimating the unknowns in a set of linear equations which exceeded the unknowns in number, assuming that the unknowns are to be estimated by linear functions of the observations. He was quite familiar with what we should now call the standard errors of partial regression coefficients. This seems very simple now, but in a pioneer it needed exceptional insight and great analytical mastery.

Gauss's first proof of the method of least squares was published in a work called *A theory of the motion of the Heavenly Bodies**, published in 1809, and he gave a second proof about 1822 in his *Theory of the combination of observations**. In the latter he adopted Laplace's ideas to a considerable extent. We have hardly the space here to do justice to Gauss's work.

Thus we see that by the end of the eighteenth century the basis of modern statistical method already existed. The crop had been sown and it was already beginning to sprout. But the harvest was not to be reaped until more than a century later—in the statistical work of the last thirty years. During the nineteenth century the theory of errors of observation was being applied in astronomy; towards the end statistical mechanics was being evolved by the physicists, and the actuaries were steadily developing their technique. But the impetus of the modern enthusiasm for statistical methods, especially in the biological and social sciences, came from another direction, not primarily mathematical at all.

It is true that Quetelet, who was born in 1796, started out as a mathematician; he became director of the Royal Observatory at Brussels in 1828. His two principal works, however, were *Sur l'homme et le développement de ses facultés, ou essai de physique sociale* (1835) and *L'anthropométrie ou mesure des différentes facultés de l'homme* (1871). These were inspired by his statistical interest as such; in them he developed his conception of "*l'homme moyen*", which we should perhaps translate as the "average" rather than the "mean" man. In the second work we find what I think is the earliest use of the normal curve in anthropology†. Quetelet was tutor to the Prince Consort and so exercised much influence on the statistics of this country. He was the real instigator of the foundation of Section F of the British Association dealing with Economic Science and Statistics, and he inspired with an enthusiasm for statistics, which amounted almost to religious fervour, one of the greatest women of the Victorian era—Florence Nightingale.

* I have translated the titles from the Latin.

† Quetelet had certainly done this much earlier. Since writing the above I have found an example of a normal curve fitted to the chest circumferences of 5738 Scottish soldiers in his *Lettres sur la Théorie des Probabilités* published in 1846.

But it was from Galton rather than from Quetelet that the impetus towards the renewed study and the renewed enthusiasm for mathematical statistical methods was ultimately to come. Galton was not a mathematician in the usual sense of the word at all. He was born in 1822 and died in 1911. From the first he was of independent means; he studied medicine, without however qualifying, and between 1845 and 1851 was engaged in African exploration in the Sudan and Damaraland. He then turned his attention to meteorology, and was one of the first to advocate systematic weather charting and to give a theory of cyclones. His *Metereographica* was published in 1863. Inspired by his cousin Charles Darwin's *Origin of Species* in 1859, he turned from climate and meteorology to the study of anthropology and race improvement. His interest in anthropology led to important researches in photography and portraiture and in psychology; it led also to his work on finger-prints, which formed the basis of the modern system of criminal identification. Statistics he regarded as fundamental to the study of anthropology in general and heredity in particular. Of all this versatility Karl Pearson says: "We have seen how Galton grew from traveller to geographer, from geographer to ethnologist, from ethnologist to anthropologist, and now the last stage appears: he is chiefly interested in anthropometry, because of the contributions he expects from it to heredity; the anthropologist becomes a geneticist. Looked at superficially, Galton's work seems like a comprehensive but confused mosaic of many branches of science. Studied in relation to his life, we see a definite pattern, a picture of long-continued mental developments; each branch of knowledge he acquired fell into its fitting place and formed a stepping-stone to further advance".

Galton loved counting things. I must here quote Pearson again: "Influenced by his own motto, 'Whenever you *can, count*'; he never went for a walk or attended a meeting or lecture without counting something. If it was not yawns or fidgets, it was the colour of hair, or eyes, or skins. But the record of several characters involves a considerable effort of memory, and using a pencil invites attention to the work of the recorder. The Galton Laboratory possesses no less than five implements of a type which Galton later termed 'registrators'. One consists of a pair of cotton gloves; on the palm or face of one glove across the fingers is a pocket capable of containing a card, about the size of a gentleman's visiting card; just below the tip of the thumb is a thin piece of wood or metal sewn into the inside of the glove and carrying a needle point projecting very slightly through the material of the glove. If the thumb be pressed against the palmar surface of any one of the four fingers a fine hole is recorded on the card. A great many holes may be pricked at haphazard close together without their running into one another or otherwise making it difficult to count them afterwards."

"The most complete registrator was one made for Galton by

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Hawkesley—the needle point is done away with, and the instrument records on five dials the number of separate pressures on five pins. These pins or stops communicate by a ratchet with a separate index-arm that moves round its own dial. The dials are covered by a plate which can be removed to read off the results. The instrument is $\frac{1}{4}''$ thick, $4''$ long and $1\frac{3}{4}''$ wide and it can be held unseen in either hand with a separate finger and thumb on each stop. When any finger is pressed on the stop below it, the corresponding index-arm records a unit. Guides are placed to keep the fingers in their proper positions. The instrument may be used in the pocket or under a loose glove or other cover. It is possible by its means to take anthropological statistics of any kind among crowds of people without exciting observation, which it is otherwise exceedingly difficult to do." "I may remark", says Pearson, "that it requires some little training to press with the correct finger." "With an instrument of this kind Galton recorded the percentage of attractive, indifferent and repellent looking women he met in his walks through the streets of various towns with the object of forming a 'Beauty-map' of the British Isles—a project he never completed, although he held London to have most and Aberdeen fewest beautiful women of the towns he had observed. He once also remarked to me that he had found Salonika to be the centre of gravity of lying, though I have no direct evidence that he used a registrator to tick off liars and truth speakers in his travels through Greece."

So much for the man. What were his contributions to statistical method? From the mass of material available in Karl Pearson's three-volumed *Life of Galton*, in its way one of the most wonderful biographies ever written, we can only pick them out in their barest outline.

Galton's main method was what Pearson calls the method of grades and deviates. If we imagine a random sample of, say, 100 men placed in order of height, with the shortest on the left and the tallest on the right, with the same distance between each man, then the tops of their heads will form an ogive curve (at least approximately); and the distance that any man stands from the left will be proportional to the number of men shorter than he. This is the basis of Galton's idea: the height of the middle man (or, if the number be even, something between the height of the two middle men) will give the median; the heights of the individual standing 25 per cent. of the way from either end will give the quartiles; and so on. As we know now stature is normally distributed, and Galton's ogive was really an integrated normal frequency curve. Many anthropometric measurements are (at any rate approximately) normally distributed, and so Galton was able to construct tables giving the deciles for a great variety of physical measurements. (In the above example the deciles would be the men dividing the base line into ten equal parts.) He started his anthropometric laboratory in 1884 at the International Health Exhibition, South Kensington, to

get such measurements: height, weight, span, breathing power, strength of pull and squeeze, quickness of blow, hearing, seeing, colour sense, and other personal data. The laboratory went on at South Kensington until 1891, and 3678 persons were measured. Galton found that males had a better record than females in most of the characters measured; one lady, however, had a squeeze of 86 lbs. This pleased *Punch*, who published the following:

THE SQUEEZE OF 86.

Maiden of the mighty muscles
  There recorded, you would be
Famous in all manly tussles,
  And it's very clear to me
That if in the dim hereafter,
  Any husband should play tricks,
You would with derisive laughter
  Give a squeeze of 86.

Husbands, be it sadly stated,
  Have been known their wives to whack;
You, unless you're overrated,
  Could give such endearments back.
Yours the task to try correction
  Till your husband and your "chicks"
Had a lively recollection
  Of the squeeze of 86.

The two main achievements of Galton's anthropometric laboratory, Pearson thinks, were these:

(i) An immense amount of material was collected, which only forty years later is being adequately reduced.
(ii) From small portions of it Galton deduced the foundations of the correlational calculus.

His first correlation table dealt with the seeds of mother and daughter plants (cress seeds); he uses five groups for each, which he calls $-2, -1, 0, 1, 2$ corresponding to the median, the two quartiles and the two points $\frac{1}{12}$ of the way from either end, and he observes the association in size between mother and daughter plants. In his memories he says: "As these lines are being written, the circumstances under which I first clearly grasped the important generalisation that the laws of heredity were solely concerned with deviations expressed in statistical units [for Galton, the quartile values] are vividly recalled to my memory. It was in the grounds of Naworth Castle, where an invitation had been given to ramble freely. A temporary shower drove me to seek refuge in a reddish recess in the rock by the side of the pathway. There the idea flashed across me, and I forgot everything else for a moment in my great delight". This was in 1888.

Galton reached the correlation coefficient via the idea of regression. He was comparing the diameters of two generations of sweet-

pea seeds, arranging the parent seeds in seven different groups and working out the mean size of the seeds of the offspring in each group, and he succeeded in obtaining what was substantially the modern expression for the regression coefficient, giving the average change in size of offspring seeds for unit change of size in parent seeds. He found that on the average the mean size of the offspring of large seeds, although greater than the mean of the race, did not differ from it as much as the mean size of their parents had done; hence the name regression: subsequently he found the same principle held for a large number of physical characteristics in man.

Having obtained the regression coefficient, Galton hit upon the idea of expressing each of the two variables to be compared in terms of its own variability as a unit; he then plotted the regression line to this scale and the slope gave him the correlation coefficient. He did not use the standard deviation as a measure of variability as we should do now, but half the difference between the two quartiles, while instead of the means of the arrays he used the medians. Thus he was assuming, perhaps unconsciously, a normal distribution of the variables. He first did this in a paper sent to the Royal Society on 5th December, 1888, entitled "Co-relations and their measurement, chiefly from Anthropometric Data".

Galton's studies of regression led him to what has been called "The Law of Ancestral Heredity". It states that if we take the deviation of an individual in any character from the mean of the race, and likewise the average deviation of the parents, grand-parents, great-grand-parents, and so on, then the average deviation of the offspring will be obtained by multiplying the parental deviation by $\frac{1}{2}$, the grand-parental deviation by $\frac{1}{4}$, the great-grand-parental deviation by $\frac{1}{8}$, and so on. In other words, each parent would contribute, on the average, $\frac{1}{4}$ of his excess in, say, stature to the offspring, each grand-parent $\frac{1}{16}$, each great-grand-parent $\frac{1}{64}$, and so on. He tried to apply the same principle to eye-colour, assuming that each parent would give his or her eye-colour to $\frac{1}{4}$ of his offspring, each grand-parent to $\frac{1}{16}$, and so on, and obtained a very fair fit, on this hypothesis, to a body of data taken from 168 families.

Of course we know now that the Law of Ancestral Heredity, at any rate in this form, is not true, but the fact that it gave in several cases quite a good approximation to observed facts is most interesting and suggestive.

I have given the merest indication of Galton's statistical work, but it must suffice. Of the theory of variability and correlation he says: "It is full of interest of its own. It familiarises us with the measurement of variability, and with curious laws of chance that apply to a vast diversity of social subjects. This part of the inquiry may be said to run along a road on a high level, that affords wide views in unexpected directions, and from which easy descents may be made to totally different goals to those we have now to reach. I have a great subject to write upon, but feel keenly my literary

incapacity to make it easily intelligible without sacrificing accuracy and thoroughness ".

Galton's work inspired an outstanding mathematician and one of the greatest scientific imaginations of our age—I refer to Professor Karl Pearson. I cannot hope to do justice to Professor Karl Pearson's work *here*—quite a good short account has recently been given by Professor Burton Camp in the *Journal of the American Statistical Association.*

His series of *Mathematical Contributions to the Theory of Evolution* started in 1893 in the *Phil. Trans. Roy. Soc.* After 1904 they were continued in the *Drapers' Company Research Memoirs* of Pearson's Biometric Laboratory. The χ^2 method for testing the concordance of theoretical hypotheses with observation was published in the *Phil. Mag.* for 1900. *Biometrika* was started by Galton, Weldon and Pearson in 1901. After Galton's death in 1911 Pearson became the first holder of the chair which Galton had endowed, the chair of National Eugenics, " the study of those agencies, under social control, which may improve or impair the racial qualities of future generations, either physically or mentally ". He directed the Eugenics and Biometric Laboratories conjointly under the title " Department of Applied Statistics ". He retired in 1933.

In the works which we have mentioned Pearson developed the theory of frequency curves, of correlation and of sampling, confining himself until about the last ten years to the theory of large samples. He made numerous applications to eugenics and anthropology. To *Biometrika* alone Pearson has contributed some 1500 pages.

To Pearson is really due the spread of modern statistical methods throughout the world. The psychologists, for instance, seized upon the theory of correlation avidly, sometimes perhaps with more enthusiasm than discretion, and to-day statistical methods are being applied in almost every branch of science.

Statistical theory has been given a new turn by the researches, during the last twelve years or so, of Professor R. A. Fisher, who has succeeded Professor Karl Pearson. It is perhaps too early to assess Fisher's work, but I should say its importance lies :

(i) In the progress made with the theory of small samples, so that we can now assess the accuracy of the information obtainable from a small sample in so far as this is dependent on its size.

(ii) In the development of tests of significance in a systematic way so that they all appear as examples of one general principle and involve as a rule only one general type of distribution, the so-called " z " distribution of which the simpler distributions sometimes required are particular cases. In particular, his extension of the field in which we can use the χ^2 test for discrepancies between observation and hypothesis, and his systematisation of the underlying principles, are remarkable.

(iii) In his solution of problems of estimation independently of probabilities *a priori*.

(iv) In providing us with a new technique, the analysis of variance, for exploring the complex sources of variation which may arise in experimental work. This arose out of the practical problems of experimental technique in agriculture, and Fisher's designs for agricultural experiments are now being used, if not from China to Peru, at any rate from China to California, and that the longer way round the world. Recently applications have been made of the method in statistics concerned with industrial standardisation, and there is little doubt it will ultimately be employed wherever statistics are being applied to experimental scientific work. I know of one case where it has been used in the study of ultra-violet absorption spectra in biochemical research.

(v) In the reconciliation of the Mendelian and biometric views of heredity by working out the consequences of particulate inheritance when applied to populations at large, and in the development of statistical methods appropriate to genetics.

I realise I have given a very incomplete picture of my subject. In particular I have made no reference to the important work of Edgeworth in mathematical statistics, nor to the important contributions of the Scandinavians, Thiele, Charlier and Cramer, to the theory of frequency distributions.

I have omitted all reference to economic applications; this is partly through ignorance, and partly because a discussion of statistical method in economics would have become a discussion of the methods of treating time-series in which there is much that is very difficult and still controversial. I have also omitted any mention of the fundamental part played by statistical ideas in modern physics. This is again through ignorance; but perhaps I might remind you that in his recent address to the British Association Sir James Jeans gave us the choice of retaining the space-time framework of the universe and sacrificing determinism, or retaining determinism and sacrificing the space-time framework. It may be a matter of taste; if we accept the former, then all the phenomena of nature become statistical in character. There is still more ground than when Keynes wrote his *Treatise on Probability* for the reversal of Quetelet's aphorism with which he concludes it: "L'urne que nous interrogeons c'est la Nature" to "La Nature que nous interrogeons c'est une urne". J. O. I.

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997. A Cambridge tutor of high reputation was once trying to familiarise a beginner with the difference between na and a^n. After repeated illustration, he asked the pupil whether he saw the point. "Thank you very much, Mr. —— ", was the answer; "I now see perfectly what you mean: but, Mr. ——, between ourselves, now, and speaking candidly, *don't* you think it's a *needless refinement*?"—A. De Morgan, *Camb. Phil. Trans.*, vol. x (1864), p. 183, f.n. [Per Dr. G. J. Lidstone.]

LAGRANGE'S EQUATION.*

By R. J. A. Barnard.

Students find partial differential equations difficult not only on account of the inherent difficulties of the subject, but because of confusion, omissions, and, frequently, errors in the textbooks. To take an illustration, Piaggio (p. 147, new edition), begins with the statement that the equations

$$\frac{dx}{P}=\frac{dy}{Q}=\frac{dz}{R}, \qquad \text{.............................}(1)$$

and $$Pp+Qq=R, \qquad \text{.............................}(2)$$

are equivalent because they represent the same surfaces.

Now the equations (1) represent curves, and any solution *must* consist of *two* equations giving surfaces whose intersections give the lines represented by the differential equations (1). To say that if $u(x, y, z)=a$ and $v(x, y, z)=b$ are two equations derived from (1), then $f(u, v)=0$ is the general solution is a statement that appears in various English textbooks but is apparently meaningless.

Let us take a very simple example of Lagrange's equation

$$p+mq=0. \qquad \text{.............................}(3)$$

We find several methods of solution given in such books as Forsyth and Piaggio. Some of these give a complete integral

$$z=a(y-mx)+b, \qquad \text{.............................}(4)$$

and some the general integral

$$z=f(y-mx).$$

But it should be pointed out that such solutions as

$$z=a(y-mx)^r+b(y-mx)^s,$$

or $$z=b\sin\{(y-mx)/a\},$$

have just as much right to the term Complete Integral as (4) has and therefore that the term *the* Complete Integral is a misnomer, while such a solution as

$$z=a(y-mx)^2+b(y-mx)+c$$

is not a complete integral though it is a special case of the general integral.

A method of deducing the general integral from a complete integral is given in the text-books for all standard types of first-order equations, but it is only in Lagrange's equation that this can be carried out. Further, it should be noticed that in this equation a complete integral is always a special case of the general integral.

Singular solutions of Lagrange's equation are seldom investigated. Since the simplest complete integral is of the form

$$u=av+b,$$

* Extracts from a paper on *Differential Equations involving Three Variables* read at the Victorian Branch of the Mathematical Association.

it is generally assumed that there is no singular solution. But this is only necessarily the case if P, Q, R are single-valued. The neglect of the case when P, Q, R are many-valued has led to unfortunate results.

For example, take the equation

$$p+q=2\sqrt{z}. \qquad \text{.................................(5)}$$

The subsidiary equations are

$$\frac{dx}{1}=\frac{dy}{1}=\frac{dz}{2\sqrt{z}}, \qquad \text{.................................(6)}$$

giving
$$x-y=A,$$
$$x-\sqrt{z}=B,$$

and a complete integral is therefore

$$x-\sqrt{z}=a(x-y)+b \qquad \text{.................................(7)}$$

and the general integral

$$x-\sqrt{z}=f(x-y). \qquad \text{.................................(8)}$$

Now (7), if rationalized, becomes

$$z=\{x-b-a(x-y)\}^2, \qquad \text{........................(9)}$$

and the ordinary method of obtaining the envelope gives

$$z=0. \qquad \text{.................................(10)}$$

The equation (9) represents parabolic cylinders touching $z=0$ along the generating line through the vertices of the principal parabolic sections. This line varies in position with a and b, so that $z=0$ is a true envelope. Further it is deducible from (5) in the usual way, for, rationalizing, we have

$$(p+q)^2=4z,$$

and differentiating separately with respect to p and q, we again get $z=0$. Consequently $z=0$ is a perfectly ordinary singular solution representing the envelope. Yet Piaggio calls it a "Special Integral" and says that it cannot be deduced from the differential equation or from the given complete integral in the usual way. He forgets apparently that exactly the same thing applies to ordinary differential equations. The general theory applies only to rationalized equations. Thus the equation

$$\frac{dy}{dx}=\sqrt{\frac{y}{x}} \qquad \text{.................................(11)}$$

has the primitive

$$\sqrt{y}-\sqrt{x}=\sqrt{c}, \qquad \text{.................................(12)}$$

but the envelope $xy=0$ cannot be obtained in the usual way from either (11) or (12). But if we rationalize we get

$$xp^2=y,$$

and
$$x^2+y^2-2xy-2cx-2cy+c^2=0,$$

and by differentiation each gives the singular solution as usual.

Forsyth (*Treatise on Differential Equations*, 5th edition, p. 383) gives as an example of a special integral one where the supposed special integral is only a particular case of the complete integral given, when one of the arbitrary constants tends to an infinite value.

Bateman, however, gives an example of a different type. We may take a similar but simpler one,

$$(z-y)p+q-1=0. \qquad \text{.......................(13)}$$

A complete solution is

$$z(z-y)-x+a(z-y)+b=0. \qquad \text{.................(14)}$$

Here $$z-y=0 \qquad \text{...............................(15)}$$

is a solution and certainly cannot be obtained in the usual way from the differential equation. What is its nature ?

The surfaces (14) are conicoids having (15) as a common tangent plane at infinity. Thus while the plane touches all the conicoids it touches them all in a line at infinity and can hardly be called an envelope. It may, in addition, be regarded as a particular case of the complete integral (14) when a is infinite. The same applies to Bateman's example.

This case can also be exactly paralleled in ordinary differential equations. If we solve the equation

$$(x^2-y^2)\frac{dy}{dx}=2xy, \qquad \text{...............................(16)}$$

we get $$x^2+y^2=2cy. \qquad \text{...............................(17)}$$

Now $$y=0 \qquad \text{...............................(18)}$$

satisfies the differential equation, but is not obtainable in the usual way for a singular solution from either the differential equation or its integral. The line $y=0$ touches all the curves at the origin (a singular point of the differential equation), but cannot be termed an envelope (see Piaggio, *Differential Equations*, new edition, 1929, p. 192). On the other hand, (18) may be regarded as a special case of (17) when c tends to infinity.

Thus in all cases the special integrals given in the textbooks seem to be either ordinary envelopes or special cases of a complete integral obtained by making one of the constants infinite. Special integrals might well be banished again from the textbooks. R. J. A. B.

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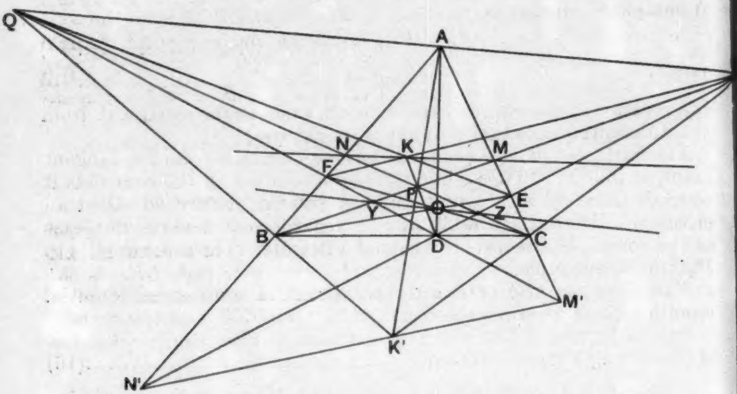
998. The Riemann and the Lebesgue integrals.

To illustrate the two processes we may also consider the land revenue collection and the collection of income tax revenue in a country. The land revenue is levied and collected plot by plot and taluk by taluk. The income tax on the other hand is levied on persons according to their income. Incomes are divided into intervals as it were, *e.g.* Rs. 1000 to Rs. 2000, Rs. 2000 to Rs. 4000, etc., and accordingly charged, and the persons having incomes falling within these ranges are picked out and assessed ; thus the collection of income tax revenue is like Lebesgue Integration.—P. V. Seshu Aiyar, " Lebesgue's Theory of Integration ", *The Mathematics Student*, vol. i, No. 4 (December 1933), pp. 135-136. [Per Prof. E. H. Neville.]

HAMILTON'S EXTENSION OF FEUERBACH'S THEOREM.

By N. M. Gibbins.

The following treatment is perhaps easier than, and is in some respects an expansion of, that given by Professor H. F. Baker in his *Principles of Geometry*, vol. ii, pp. 58-60.



1. A, B, C, O are four given points; AO meets BC in D, BO meets CA in E, CO meets AB in F. Then DEF is the diagonal point triangle of the quadrangle $ABCO$. Take this as triangle of reference for areal coordinates and let O be (u, v, w). Then A is $(-u, v, w)$, B is $(u, -v, w)$ and C is $(u, v, -w)$. Also the equations of BC, CA, AB are respectively $y/v+z/w=0$, $z/w+x/u=0$, $x/u+y/v=0$; and the equation of the axis of perspective of ABC and DEF is $x/u+y/v+z/w=0$—the line $N'M'$.

2. An infinite number of quadrilaterals can be described whose sides pass through A, B, C, O respectively and which have DEF as diagonal line triangle. For take any line $lx+my+nz=0$ through O, so that $lu+mv+nw=0$, and let it meet DF in Y and DE in Z. Let BY meet DE in R and let CZ meet DF in Q. Let BR and CQ meet in P. Then the equation of RP is $lx-my+nz=0$, and that of PQ is $lx+my-nz=0$. Hence P is on FE. Further, the line whose equation is $-lx+my+nz=0$ passes through Q, A and R.

3. Using the relation $lu+mv+nw=0$, the equation

$$l^2ux+m^2vy+n^2wz=0$$

may be written in the three following ways:

$$lu(-lx+my+nz)+mn(wy+vz)=0\ ;$$
$$mv(lx-my+nz)+nl(uz+wx)=0\ ;$$
$$nw(lx+my-nz)+lm(vx+uy)=0.$$

Hence it is the equation of the axis of perspective of ABC and PQR, that is, the line NM in the figure. Hence also PQR is the diagonal line triangle of the quadrilateral BC, CA, AB, NM.

4. DP, EQ, FR meet in the point given by $lx=my=nz$ which is on NM; that is, the centre of perspective K of DEF and PQR is on the axis of perspective of ABC and PQR. It may be verified that AP, BQ, CR meet in the point (lu^2, mv^2, nw^2) which is on $N'M'$. Hence the centre of perspective K' of ABC and PQR is on the axis of perspective of ABC and DEF. Also the diagonal point triangle of $PQRK'$ is ABC.

5. The equation of a conic having PQR as a self-conjugate triangle is

$$\Omega\equiv\alpha(-lx+my+nz)^2+\beta(lx-my+nz)^2+\gamma(lx+my-nz)^2=0.$$

The conic whose equation is

$$\Sigma\equiv\Omega-(\alpha+\beta+\gamma)(l^2ux+m^2vy+n^2wz)(x/u+y/v+z/w)=0$$

passes through the points D, E, F, since the coefficients of x^2, y^2, z^2 are all zero. Hence if one chord of intersection of a conic having PQR as a self-conjugate triangle and a conic through DEF is the axis of perspective of PQR and ABC, then the other chord of intersection is the axis of perspective of ABC and DEF, and *vice versa*.

This is Professor Baker's generalization of Hamilton's extension of Feuerbach's theorem.

6. By the last sentence of § 3, PQR is a self-conjugate triangle for all conics which touch the lines BC, CA, AB, NM. Hence if a conic through D, E, F touches NM at the same point as one of these conics, their other chord of intersection is $N'M'$.

7. Conversely, if A, B, C are three points and L a line, let D on BC be the harmonic conjugate with respect to B and C of the point in which L meets BC; and similarly for E and F. Let any conic touching BC, CA, AB cut L in the points I and J; then the conic $DEFIJ$ touches this conic. Here the line L is given, but not the points I and J on it.

8. If, however, A, B, C, I, J are all given, there are only four conics which pass through I and J and touch BC, CA, AB. It is now possible to find a point T such that each of the line pairs AT, BC; BT, CA; CT, AB meets the line IJ in points harmonically conjugate with respect to I and J; and in that case I and J are conjugate points with respect to the pencil of conics $ABCT$.

Now I, J, D, E, F are five of the well-known eleven points on the locus of the poles of IJ with respect to the pencil. Hence the conic $IJDEF$ is the locus of the poles and passes through the other six points, in particular U, V, W—the vertices of the diagonal point triangle of the quadrangle $ABCT$. If then I and J are two points conjugate with respect to a pencil $ABCT$, the conic $IJUVW$ touches each of the four conics which pass through I and J and touch BC, CA, AB. Furthermore, each of the points A, B, C, T is the "harmocentre" with respect to I and J of the triangle formed by the

other three. Hence the conic $IJUVW$ touches each of the sixteen conics which pass through I and J and touch the joins of any three of the four points A, B, C, T.

9. We may now either state the theorems correlative to those of 5-8, or else deduce them directly from consideration of the equation

$$\alpha(-\lambda u+\mu v+\nu w)^2+\beta(\lambda u-\mu v+\nu w)^2+\gamma(\lambda u+\mu v-\nu w)^2$$
$$-(\alpha+\beta+\gamma)(\lambda l u^2+\mu m v^2+\nu n w^2)(\lambda/l+\mu/m+\nu/n)=0,$$

in which the coefficients of λ^2, μ^2, ν^2 are each zero, and where λ, μ, ν denote tangential coordinates corresponding to areal coordinates.

N. M. G.

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999. Now all the world understands the irresistible force that compels the poet, at last, to give form to long haunting dreams; the need, also, of the astronomer to crystallize the results of his discoveries and formulate his epoch-making theories; but the passion of the mathematician to do the same is not so easily comprehensible. For years Baltazar had dreamed of an exhaustive and monumental treatise on the Theory of Groups which would revolutionize the study of the higher mathematics, a gorgeous vision, the mere statement of which must leave the ordinary being cold and the first attempt at explanation petrify him with its icy unintelligibility. This dream was now in process of accomplishment. He had also to put into form fascinating adventures into the analytical geometry of the ghostly and unrealizable space of Four Dimensions. There, he was wont to assert, you entered the true Fairyland of mathematics.—W. J. Locke, *The House of Baltazar*, chap. iv. [Per Mr. R. O. Street.]

1000. After his tea and cold tub, he sat down to the table by the eastern window through which the morning sun was streaming, and attacked the final chapter of his epoch-making Treatise on the Theory of Groups. The thrill of a great thing accomplished held him as he wrote. Such moments were worth living. He breakfasted with the appetite of a man who had a right to the material blessings of life. He went out, groomed the old grey mare and cleaned out the stable and dug up a patch of ground, rejoicing, like a young man, in his strength and in the fresh beauty of the day. On his return to his study he reviewed affectionately the monuments of two years' labour.... The Treatise on the Theory of Groups, all but complete, lay in one neat pile of manuscript. Another represented further serious adventures into the Analytical Geometry of a Four-Dimensional Space than mortal man had ever undertaken. Who could tell whither these adventures could lead? Pure mathematics had demonstrated the existence of the planet Neptune in space of three dimensions. Pure mathematics applied to four dimensions might prove and explain many transcendental phenomena. The next world might be four-dimensional, and the spirits of the dead who inhabit it could easily enter confined three-dimensional space. That was Cayley's ingenious theory of Ghosts. You could carry it further to space of five, six, *n* dimensions; when you could treat the geometry of space of infinite dimensions as Euclid did the geometry of plain surfaces, you would have solved the riddle of the universe; you would have come direct to the Godhead.—W. J. Locke, *The House of Baltazar*, chap. vi. [Per Mr. R. O. Street.]

1001. Monsieur, les gens du monde traitent toujours la Science assez cavalièrement, tous nous disent à peu près ce qu'un incroyable disait à Lalande en lui amenant des dames après l'éclipse : " Ayez la bonté de recommencer ".—H. de Balzac, *La Peau de Chagrin*. [Per Mr. J. B. Bretherton.]

THE COEFFICIENTS OF THE SERIES FOR $\tan x$.

BY E. F. SIMONDS.

IN the *Gazette* of October, 1928 (Vol. XIV, p. 233), Professor H. G. Forder gives a simple inductive proof of the theorem on the Euler Numbers :

$$E_{n+2} - E_n \equiv 0 \pmod{60}.$$

He remarks, however, that his method is not successful in proving completely the allied theorem for the coefficients of the tangent series

$$T_{n+2} - T_n \equiv 0 \pmod{90}.$$

The following is a method of proving the latter theorem ; it applies equally well to the former.

An alteration in the notation is convenient ; T_n will be changed to T_{2n-1}.

Let
$$\tanh x \equiv \frac{e^x - e^{-x}}{e^x + e^{-x}} = T_1 x + \frac{1}{3!} T_3 x^3 + \frac{1}{5!} T_5 x^5 + \dots \dots \dots (1)$$

Consider the series $1 + T_1 x + \frac{1}{2!} T_2 x^2 + \frac{1}{3!} T_3 x^3 + \dots$,

where T_1, T_3, T_5, ... have the same values as in (1), while

$$T_2 = T_4 = T_6 = \dots = 0.$$

This series can be represented symbolically by e^{Tx}, on the understanding that T^k is to be replaced by T_k. It can readily be shown that the law of multiplication holds symbolically, *i.e.*

$$e^{Tx} . e^{ax} = e^{(T+a)x}.$$

Equation (1) may now be written

$$(e^x - e^{-x})/(e^x + e^{-x}) = \tfrac{1}{2}(e^{Tx} - e^{-Tx}),$$

whence we get, on multiplication,

$$e^x - e^{-x} = \tfrac{1}{2}(e^{(T+1)x} - e^{-(T+1)x}) + \tfrac{1}{2}(e^{(T-1)x} - e^{-(T-1)x})$$

$$= \left[(T+1)x + \frac{1}{3!}(T+1)^3 x^3 + \dots \right] + \left[(T-1)x + \frac{1}{3!}(T-1)^3 x^3 + \dots \right].$$

Equating coefficients of x^{2n+1} we get

$$(T+1)^{2n+1} + (T-1)^{2n+1} = 2. \dots \dots \dots (2)$$

Giving n the values 0, 1, 2, ... , we find in succession

$$T_1 = 1,\ T_3 = -2,\ T_5 = 16,\ T_7 = -272,\ T_9 = 7936, \dots ,$$

and we observe that $T_7 - T_3 \equiv T_9 - T_5 \equiv 0 \pmod{90}$.

Our problem now is to show that for $n > 2$

$$T_{2n+1} - T_{2n-3} \equiv 0 \pmod{90},$$

and we proceed by induction, assuming that we know

$$T_7 - T_3 \equiv T_9 - T_5 \equiv \dots \equiv T_{2n-1} - T_{2n-5} \equiv 0 \pmod{90}.$$

Equation (2) is equivalent to

$$T_{2n+1}+c_2T_{2n-1}+c_4T_{2n-3}+\ldots+c_{2n-4}T_5+c_{2n-2}T_3+c_{2n}T_1=1, \ldots(3)$$

where the c's are coefficients of $(1+x)^{2n+1}$. In (3), put

$$T_1=t_1,\ T_3=t_3,\ T_5-T_1=t_5,\ T_7-T_3=t_7,\ T_9-T_5=t_9, \text{ etc.};$$

and we get, assuming that

$$t_{2n-1}\equiv t_{2n-3}\equiv\ldots\equiv t_7\equiv 0 \pmod{90},$$

and remembering that $t_5=15,\ t_3=-2,\ t_1=1,$

$$t_{2n+1}+16(1+c_4+\ldots+c_{2n-4}) \\ -2(c_2+c_6+\ldots+c_{2n-2})+c_{2n}\equiv 1 \pmod{90} \ldots\ldots(4)$$

if n is even; or

$$t_{2n+1}+16(c_2+c_6+\ldots+c_{2n-4}) \\ -2(1+c_4+\ldots+c_{2n-2})+c_{2n}\equiv 1 \pmod{90} \ldots\ldots(5)$$

if n is odd.

We shall now show that

$$t_{2n+1}\equiv 0 \pmod{90}\equiv 0 \pmod{2\,.\,9\,.\,5}.$$

To show that $t_{2n+1}\equiv 0$ (*mod* 2).

This is obvious, for $c_{2n}\equiv 2n+1$.

To show that $t_{2n+1}\equiv 0$ (*mod* 9).

If we write $16\equiv -2 \pmod 9$, both equations (4) and (5) reduce to

$$t_{2n+1}-2\,.\,2^{2n}+3(2n+1)\equiv 1 \pmod 9.$$

Now n is of the form $3k$, $3k+1$, or $3k+2$.

(*a*) Let $n=3k$; then

$$t_{2n+1}-2\,.\,2^{6k}+3(6k+1)\equiv 1 \pmod 9.$$

But $2^6\equiv 1 \pmod 9$; hence $2^{6k}\equiv 1 \pmod 9$.

Thus $t_{2n+1}-2+3\equiv 1 \pmod 9$,

and hence $t_{2n+1}\equiv 0 \pmod 9$.

(*b*) Let $n=3k+1$.

Then $t_{2n+1}-2\,.\,2^{6k+2}+3(6k+3)\equiv 1 \pmod 9$.

Thus $t_{2n+1}-2^3\equiv 1 \pmod 9$,

and so $t_{2n+1}\equiv 0 \pmod 9$.

(*c*) Let $n=3k+2$.

Then $t_{2n+1}-2\,.\,2^{6k+4}+3(6k+5)\equiv 1 \pmod 9$.

Thus $t_{2n+1}-2^5+6\equiv 1 \pmod 9$,

and so $t_{2n+1}\equiv 0 \pmod 9$.

To prove that $t_{2n+1}\equiv 0$ (*mod* 5).

In equations (4) and (5) put $16\equiv 1 \pmod 5$, and get

$$t_{2n+1}+(1+c_4+\ldots+c_{2n-4}+c_{2n}) \\ -2(c_2+c_6+\ldots+c_{2n-2})\equiv 1 \pmod 5 \ldots\ldots\ldots\ldots(6)$$

if n is even ; or

$$t_{2n+1}+(c_2+c_6+\ldots+c_{2n})$$
$$-2(1+c_4+c_8+\ldots+c_{2n-2})\equiv 1 \pmod 5 \quad \text{............(7)}$$

if n is odd.

Now $\quad (1+c_4+c_8+\ldots)=2^{2n-1}+\epsilon 2^{n-1}$,

and $\quad (c_2+c_6+c_{10}+\ldots)=2^{2n-1}-\epsilon 2^{n-1}$,

where $\epsilon=+1$ if $n=4k$ or $4k+3$, and -1 if $n=4k+1$ or $4k+2$.

(*a*) $n=4k$ (use equation 6) ;

$$t_{2n+1}+(2^{2n-1}+2^{n-1})-2(2^{2n-1}-2^{n-1})\equiv 1 \pmod 5.$$

Hence $\quad t_{2n+1}-2^{8k-1}+2^{4k-1}+2^{4k}\equiv 1 \pmod 5$.

But $\quad 2^4\equiv 1 \pmod 5$.

Therefore $\quad t_{2n+1}-2^{8k-4}\,.\,2^3+2^{4k-4}\,.\,2^3+2^{4k}\equiv 1 \pmod 5,$

$$t_{2n+1}\equiv 0 \pmod 5.$$

(*b*) $n=4k+1$ (use equation 7) ;

$$t_{2n+1}+(2^{2n-1}+2^{n-1})-2(2^{2n-1}-2^{n-1})\equiv 1 \pmod 5.$$

Thus $\quad t_{2n+1}-2^{8k+1}+2^{4k}+2^{4k+1}\equiv 1 \pmod 5,$

whence $\quad t_{2n+1}-2+1+2\equiv 1 \pmod 5,$

and finally $\quad t_{2n+1}\equiv 0 \pmod 5.$

and similarly for the other two cases. E. F. Simonds.

. . . .

BUREAU FOR THE SOLUTION OF PROBLEMS.

This is under the direction of Mr. A. S. Gosset Tanner, M.A., Derby School, Derby, to whom all inquiries should be addressed, accompanied by a stamped and addressed envelope for the reply. Applicants, who must be members of the Mathematical Association, should wherever possible state the source of their problems and the names and authors of the textbooks on the subject which they possess. As a general rule the questions submitted should not be beyond the standard of University Scholarship Examinations. Whenever questions from the Cambridge Scholarship volumes are sent, it will not be necessary to copy out the question in full, but only to send the reference, *i.e.* volume, page, and number. The names of those sending the questions will not be published.

1002. As abstract mathematics require no collateral aid, they may reach the highest perfection in ages of general barbarism ; and there seems to be no reason why, if the course of study had been directed that way, there should not have arisen a Newton or a La Place, instead of an Aquinas or an Ockham. The knowledge displayed by Roger Bacon and by Albertus Magnus, even in the mixed mathematics, under every disadvantage from the imperfection of instruments and the want of recorded experience, is sufficient to inspire us with regret that their contemporaries were more inclined to astonishment than to emulation. These inquiries indeed were subject to the ordeal of fire, the great purifier of books and men ; for if the metaphysician stood a chance of being burned as a heretic, the natural philosopher was in not less jeopardy as a magician.—H. Hallam, *View of the state of Europe during the Middle Ages*, (1860), iii, pp. 432-433.

MATHEMATICAL NOTES.

1128. *Inverted practice: an extension of "Russian peasant" multiplication.*

Given a whole number M and any sequence of numbers $r_1, r_2, r_3, \dots$, construct a sequence of quotients $M_1, M_2, M_3, \dots$ and remainders $a_1, a_2, a_3, \dots$, so that identically

$$M = r_1 M_1 + a_1, \quad M_1 = r_2 M_2 + a_2, \quad M_2 = r_3 M_3 + a_3, \dots .$$

Then

$$M = a_1 + a_2 r_1 + a_3 r_2 r_1 + a_4 r_3 r_2 r_1 + \dots ,$$

and if N is another number,

$$MN = a_1 . N + a_2 . r_1 N + a_3 . r_2 r_1 N + a_4 . r_3 r_2 r_1 N + \dots ,$$

that is,

$$MN = a_1 N + a_2 N_1 + a_3 N_2 + a_4 N_3 + \dots$$

where

$$N_1 = r_1 N, \quad N_2 = r_2 N_1, \quad N_3 = r_3 N_2, \dots .$$

Arranging the calculations in a tabular form:

	M	a_1	N	$a_1 N$
r_1	M_1	a_2	N_1	$a_2 N_1$
r_2	M_2	a_3	N_2	$a_3 N_2$
r_3	M_3	a_4	N_3	$a_4 N_3$
. .	. .	. .	. .	. .
. .	. .	. .	. .	. .

we can compute MN as the sum of the numbers in the last column.

Prof. Lodge puts the argument more simply. If $M = M_1 r_1 + a_1$, we calculate MN as $M_1 r_1 N + a_1 N$, that is, as $M_1 N_1 + a_1 N$, where $N_1 = r_1 M$; setting aside $a_1 N$ to be added in at the end, we deal with the second product $M_1 N_1$ in the same way. "A kind of inverted practice" is Prof. Lodge's apt description of the process.

The calculation is most economical when each of the remainders is either 0 or 1, for then the fifth column is merely a selection from the fourth, and instead of writing down the terms twice, we omit them from the fourth column if they are wanted in the fifth. This simplification is assured if each of the divisors $r_1, r_2, r_3, \dots$, is 2, that is, if what we are really doing is to express M in the binary scale. But then a remainder is 0 or 1 according as the number which is divided is even or odd, and since we can recognize this distinction at sight we need not insert the column of remainders. Thus we have the familiar trick sometimes called "Russian peasant multiplication": divide M again and again by 2, ignoring the remainders and writing down the quotients; double N again and again; the product MN is the sum of those multiples of N that correspond to the quotients of M that are odd, N itself being included if M itself is odd.

The powers of 2 grow slowly, and we may ask whether the trick can be worked a little faster without serious complication. We remark that there is nothing in the identities to restrict the remainders to be positive. We can work in the scale of 3 and limit each remainder to one of the values 0, ± 1; at the cost of separating positive and negative terms for summation, we reduce the number of steps to about two-thirds of the number in the scale of 2. But since we shall be dividing and multiplying by 3, to avoid multiplication by 2 is artificial, and we might as well keep to the ternary scale in its simplest form and avoid subtraction.

The scale of 4 seems to have little to recommend it. But dividing and multiplying by 5 are effectively the same operations as multiplying and dividing by 2, and if negative remainders are permitted the remainders can be limited to the set 0, ± 1, ± 2. That is to say, while working in the scale of 5, and so reducing the number of steps to about three-sevenths of the number in the scale of 2, we still have no individual operations but doubling, halving, adding, and subtracting. If we are to work to a scale, this is perhaps the limit of efficiency in the method, for if we are going to multiply and divide by numbers other than 2 and 5 we may as well do our sums in the conventional way. But if we are prepared to vary the factor, we may use at each step any factor of one of the three forms 10^k, $2 . 10^k$, $5 . 10^k$ which happens to give one of the remainders 0, ± 1, ± 2; the largest suitable factor is always obvious. In the example added, the scale of 2 would require 20 lines and the scale of 5 would require 9, but with a varying factor 6 lines suffice; even so, twice as many figures are written down as in the equally rapid and slip-proof "Hindoo" method of long multiplication without carrying, which is perpetually being rediscovered (see, for example, *Gazette*, XI, pp. 14 and 88).

It is interesting to notice that when ordinary long multiplication is regarded as a special case of inverted practice, the digits of the multiplier are used from right to left and the sum has its old-fashioned appearance.

	5 65799	-1	12 34567	12 34567	
200	2829	-1	2469 134	2469 134	
10	283	-2	24691 34	49382 68	
5	57	2	1 23356 7		2 46913 40000
5	11	1	6 17283 5		6 17283 5
10	1	1	61 72835		61 72835
				-51864 15967	$+70$ 37031 90000

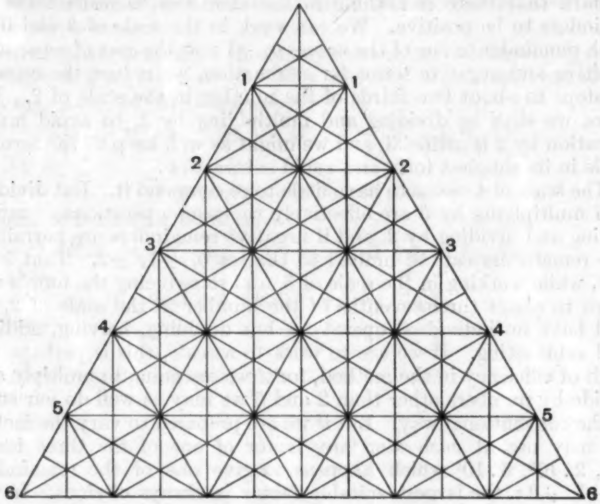
$$5\,65799 \times 12\,34567 = 69\,85167\,74033.$$

E. H. N.

1129. *A challenge.*

The ancient mathematicians used to challenge each other over the solution of what each thought to be a difficult or insoluble problem for the other. We now beg to challenge the present-day

mathematicians to solve the following problem by giving us the general solution.



The diagram represents a series of equilateral triangles, each divided by perpendiculars to the middle points of the sides. The numbers indicate the tiers; in tier (1) there are 16 triangles, in tier (2) there are 104, and in tier (3) 303. How many are there in the nth tier ?

J. TRAVERS.

1130. *Note on the decimal point.*

It is unfortunate that this point has been put in an unsymmetrical position. It stands at one side of the *units' place*, which is the centre of symmetry of a number expressed in decimal notation. One bad effect of this is apparent when one has to write the characteristic of a logarithm. Taking the decimal point as the point of reference, the rule for a positive characteristic is different from that for a negative one.

If the units' place is taken as starting point, the rule: *Characteristic equals the number of steps to the right from the leading digit to the units' place (steps to the left being reckoned negative)*, suffices for all cases.

Is it too late to shift the decimal point ? Could it be placed *below* the units' place instead of to the right of it ? This might be awkward for the printer. Other devices could be suggested; probably the easiest change to make would be to have *two* decimal points, one on either side of the units' place. This would restore symmetry and give no trouble to the printer.

Another matter concerning the decimal notation may be referred

to. It was Sir William Thomson (before he became Lord Kelvin) who first introduced the practice of writing a large or small number in the form

(a number lying between 1 and 10) $\times$ (a power of 10),

e.g., $3{\cdot}52 \times 10^6$, or $1{\cdot}78 \times 10^{-8}$, where the index of the power of ten is the characteristic of the logarithm of the number. The full benefit of this mode of writing a number is not always recognized. Compare 352000 and $3{\cdot}520 \times 10^5$. The latter shows that there are four significant digits, which is not indicated in the former.

Another historical item: it was Sir William Thomson who first suggested the use of a four-figure table of logarithms and anti-logarithms, and had one printed for the use of students in his classes.

R. F. MUIRHEAD.

1131. *A generalisation of the Frégier point.*

1. In Vol. XVIII, No. 227 (February 1934) of the *Gazette*, Mr. A. G. Walker has given an analytical proof of the latter part of the theorem:

> "The envelope of the chord of a conic S, which subtends a right angle at a fixed point O, is a conic having one focus at O; if O' is the other focus, the angle subtended by OO' at the centre of S is bisected by the axes of S",

with the apologetic remark:

> "I have not been able to find a short proof . . . by pure geometry and should be interested to hear of one".

We give below a geometrical proof by means of Gaskin's theorem.

Let the reciprocal of S with respect to O be Σ and the director circle of Σ be C; let l be the straight line midway between O and the polar of O with respect to C. Evidently, from principles of reciprocation, the envelope of chords of S which subtend a right angle at O is the reciprocal of C, having one focus at O and the other at O', the reciprocal of l.

If P, Q be the reciprocals of the axes of S, then the right-angled triangle OPQ is self-conjugate with respect to Σ and hence, by Gaskin's theorem, the circumcircle of OPQ cuts C orthogonally. By elementary geometry, the middle point of PQ lies on l. In other words, PQ is divided harmonically by l and the line at infinity. Reciprocally, the axes of S divide OO' harmonically. Since the axes are at right angles, they bisect the angle subtended by OO' at the centre of S. Thus the theorem is proved.

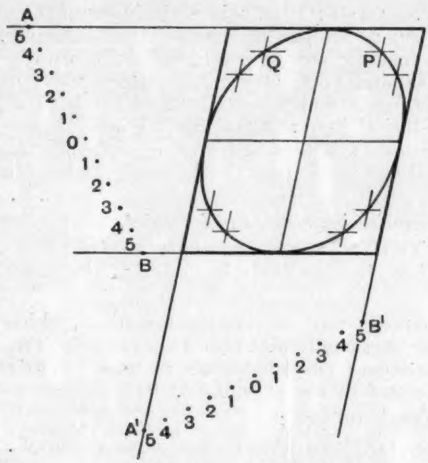
2. Incidentally, we have a simple construction for the axes of the reciprocal of a given conic Σ with respect to a given point O. Draw the circle OPQ to cut the director circle of Σ orthogonally so that the diameter PQ may be the polar of O with respect to Σ. The reciprocals of P, Q are the required axes.

3. The following allied results may also be noticed:

(i) If AOB, COD be two perpendicular chords of the conic S, equally inclined to the axes of S, then A, B, C, D lie on a circle whose centre is O'.

(ii) The envelope-conic is the Φ-conic of S and the circular lines through O. A. A. KRISHNASWAMI AYYANGAR.

1132. *To inscribe the principal ellipse in a parallelogram.*



Place a ruler as shown, AB being 2″. Prick through 10 equal divisions and project through mark 4. Repeat the process with the ruler at $A'B'$ and project through mark 3. This gives points P and Q on the ellipse, the reason being that $(3/5)^2+(4/5)^2=1$. The other points are obtained in like manner. If we use the result $(5/13)^2+(12/13)^2=1$, we may find, in a simple manner, further (and usually enough) points on the ellipse. V. NAYLOR.

1133. *The " alternate segment ".*

The proof given by G. H. G.-G. in the October *Gazette* is not as new as he thinks. I remember Prof. Alfred Lodge, then my colleague, coming into my class-room about twenty years ago to show me " the perfect proof at last ", and it was not till the next day that he detected the limitation which spoils it for the case of the acute angle, while leaving it the best available proof for the case of the obtuse angle, independent of the other case. Nor is it necessary for the two boys to have invented the proof independently, for it is given in Tuckey and Hollowell's *School Geometry* (and very likely in other available textbooks *) for the obtuse case, with an explanation of why it fails to work completely for the other ; so it is perhaps more probable that the boys had learnt the proof from some textbook. C. O. TUCKEY.

* [A correspondent refers to A. B. Mayne, *Essentials of School Geometry*, p. 234.—ED.]

REVIEWS.

Correspondance du P. Marin Mersenne, Religieux Minime. I. 1617-1627. Publiée par Mme. Paul Tannery, éditée et annotée par Cornelis de Waard avec la collaboration de René Pintard. Pp. lxiv, 668, with plates. 120 fr. 1933. Bibliothèque des archives de philosophie. (Beauchesne, Paris)

The volume before us is the first instalment of a great work which will be welcomed by all who are interested in the history of science and philosophy, and will be indispensable to all who wish to follow and appreciate the immense development of scientific and philosophical thought in the seventeenth century. The size of the undertaking might well appal any less devoted workers than those responsible for this volume. The volume itself contains over 700 large pages, of which the notes and subsidiary matter in smaller type constitute a large proportion ; and there will be twelve volumes. The first volume covers only the years 1617 to 1627. (Mersenne died in 1648.)

The idea of publishing the Mersenne correspondence originated with Paul Tannery, whose attention was drawn to the subject by the number of times he came across the name of Mersenne while preparing his great editions of the works of Fermat and Descartes. Already in 1895, writing of a project of publishing the collection of Mersenne letters preserved in the Bibliothèque Nationale, Tannery made the following observations : " It is unnecessary to insist on the interest which this correspondence possesses for the study of the intellectual movement of the seventeenth century, as well as for many other questions, of language, manners, etc. But its publication as a whole presents great difficulties, and one of the greatest is unquestionably the necessity, if one would make of it a work of real utility, of elucidating by adequate notes, the different allusions contained in the letters in question, as also of giving all desirable information about the personages and the facts mentioned therein. It is a task which the number of the correspondents and the variety of the subjects treated increase beyond all proportion ". At his lamented and premature death in 1904, Tannery left a considerable mass of papers bearing on the subject, and Mme. Tannery, with the help of Cornelis de Waard and others, is producing, as a tribute to her husband's memory, at the cost of wellnigh incredible labour, an edition which satisfies the high standard laid down by Tannery himself.

Marin Mersenne belonged to the religious Ordre des Minimes, and there was no subject in the intellectual sphere to which he was a stranger. In this volume itself we find questions discussed which belong to theology, philosophy, science in all its branches, medicine, chemistry, physics, optics, mechanics, pure mathematics, while there is much on the theory of music and the construction of musical instruments, especially the organ. Mersenne became a sort of centre of the scientific movement of the age. Without professing great originality himself, he was accessible to, and welcomed, all new ideas : " The conversation of pious and learned persons was his occupation and his delight ". The editors quote from Mersenne's friend and biographer, de Coste, a list of these " pious and learned persons " which, with their descriptions, fills some eleven pages of the note on Mersenne's life at the beginning of the volume. In this list we find (the oddly spelt) " Mr. le Chevalier d'Igby, Seigneur anglois, connu par toute l'Europe par ses excellentes qualitez, et plusieurs autres Milords et Seigneurs de ce Royaume là " (the latter included the brothers Cavendish). Thomas Hobbes was another intimate friend. " At Paris Mersenne was the common friend of savants who, very conscious of their antagonism, had no communication except through him : Beaugrand and Desargues, Descartes and Gassendi, Roberval and Hobbes ". Men like Des-

cartes, Fermat, Frénicle and Mydorge were in the habit of communicating their discoveries to Mersenne before publication.

The editors have included in the correspondence certain prefaces or dedications prefixed by Mersenne to his own works, which were very miscellaneous in character. Among them were a *Synopsis Mathematica* (1626) which seems to have included all the ancient works of importance (Euclid's *Elements*, the works of Archimedes, Apollonius' *Conics*, the *Sphaerica* of Theodosius and Menelaus, and works on optics and mechanics); *Traité de l'harmonie universelle* (1627, 1637), *Cogitata physico-mathematica* (1644), *Les méchaniques de Galilée* (1634), a translation with additions. The *Cogitata*, Hobbes tells us, " as was his custom, he did read to his friends before he sent it to the press ".

Naturally the majority of the 86 letters in the volume are letters to Mersenne, while 14 (including dedicatory letters) are from Mersenne himself. The letters deal with all sorts of subjects. For example, the first (from Claude Bredeau) discusses the incident in 1 Sam. xiv about Jonathan and the honey, when, after he had tasted the honey, his eyes were lit up (" enlightened "); this leads to quotations from Aristotle and Pliny about the medical virtues of honey; next, other questions about Saul and Jonathan are examined. The second letter (from Mersenne) returns to the question of the honey, quoting, in addition to Pliny and Aristotle, Oribasius, Galen, Dioscorides, Matthiolus; so also letter 3 quotes from Nacquert, a doctor; letter 4 refers to the same question and (with 5) discusses rabies and its treatment. 7, 8 and 9 are against atheists and heretics; 10, 11 and 13 treat at length of the theory of music (including Greek music); 37 is concerned with chemistry, 40 with the speed of light and sound, 44 with echoes, 47 with drunkenness; and so on.

Pure mathematics occupies only a small part of the volume: mathematics may be expected to come more largely into the later volumes. But documents 34 and 36 relate to the duplication of the cube or the finding of two mean proportionals. If x, y are the required mean proportionals between a, b, so that $a/x=x/y=y/b$, Menaechmus had, in Plato's time, found x and y by constructing the parabolas $x^2=ay$, $y^2=bx$. The construction in document 34 uses the parabola $y^2=bx$ and the *circle* $x^2+y^2=bx+ay$; and a note from Mersenne applauds " summus vir " (no doubt Descartes) for having invented a method of solution which required only *one* parabola (with a circle). An elaborate proof of the construction, with analysis and synthesis complete, was sent by Mydorge to Descartes (document 36). In the letter from Mydorge to Mersenne of February-March 1626 (document 52) there is what amounts to a correct statement of the law of refraction, discovered earlier by Willebrord Snellius; there was no doubt a rediscovery in Paris of the same law as the result of researches instigated by Mersenne. In the same letter there are some propositions about the ellipse and hyperbola. There is a good deal of optics in the letters following, with the ample notes thereon, particularly on the construction of conicoidal mirrors, instruments for measuring refraction and the like.

The notes are so elaborate and exhaustive that one can only call the volume a mine of information about the scientific thought of the early seventeenth century, of the extent of which it is impossible, in a short notice, to give any adequate idea. We shall look forward with the greatest interest to the appearance of the remaining volumes. T. L. H.

Isaac Newton: a biography. By Louis Trenchard More. Pp. xii, 675. 18s. 1934. (Charles Scribner's Sons, New York and London)

The celebration of the bicentenary of Newton's death led to the publication

of several books which gave accounts of his life and work; but undoubtedly the happiest outcome of the anniversary is the appearance of Professor More's biography of the "High Priest of Science". It confirms, by its publication of contemporary evidence, the long suspected inadequacy of Brewster's work, and, by its completeness, its accuracy, and its impartiality would appear likely to remain the standard account of the many-sided genius who is its subject. Professor More has had access to all the known sources, and it seems improbable that later discoveries will bring to light anything which will substantially affect our judgment of Newton's work and character.

The main facts are too familiar to require repetition, so I will refer only to those which, in view of new evidence, necessitate a revision of our previous judgments. There is nothing which alters the accepted view as to the brilliant originality and fundamental importance of his scientific discoveries. His superiority was acknowledged by his rivals, even when controversy was at its height, and, if his character appears less stainless—and less inhuman—than in the pages of Brewster, it is now possible to recognize the man as well as the scientist.

His natural diffidence was probably accentuated by the rough comradeship of the village school which he attended. It was, no doubt, very marked during his first years at Cambridge—we have the account of his disconsolate wandering when driven from his room by the rowdy habits of the man who shared it—although his carefully-kept book of expenses reveals that later he went to taverns, and even lost money at cards. It was this shyness which lay at the bottom of his antipathy to the publication of his work. He shrank from the contacts thus forced upon him, and the controversies which resulted from his first communications to the Royal Society confirmed his reluctance. Even when resident in London, and a welcome guest in many houses, he found conversation difficult, and preferred always to express his thoughts on paper. He was evidently capable in business matters; his letters reveal that he took an active interest in the management of his Woolsthorpe estate, and the fortune accumulated at his death was considerable. He was generous with his time, his money, and his influence, ever ready to assist younger men who were striving for recognition, and of scrupulous scientific integrity; but suspicious, inflexible to anyone who seemed to endeavour to rob him of the just fruits of his work, and unjust in the controversies which embittered many years of his life. It was in these that the worst side of his character was exposed, and the considerable place they occupy in the biography is necessary both on account of the important place they hold in mathematical history and for the light that they throw on Newton's character. They commenced with Newton's first major discovery, that of the composite nature of white light. Hooke, a man of great experimental skill and bold speculations, but suspicious, hasty, and without opportunity or patience to place his hypotheses on an irrefutable basis of experimental fact, claimed that the discovery was latent in his own work. Newton, for a time, carefully replied to his complaints and the criticisms of others, but ultimately in disgust refused to contend further. Hooke again attacked him when the *Principia* was published, and the breach between them became complete. In this quarrel, the chief fault of Newton was his reluctance to acknowledge any merit in, or debt to, the work of Hooke. In the controversy with Leibniz his part is considerably more disreputable. He is popularly supposed to have held aloof, until forced into action towards the end of the dispute. Professor More now shows, from Newton's own records, that he "packed the investigating committee of the Royal Society, supplied it with data and directed its enquiry; and anonymously wrote the preface for their published report, and made changes in the text". His opponents were equally

unscrupulous, and one can only regret that so unsavoury a matter could, for so long a period, occupy the energies of such eminent disputants.

But it is in connection with Flamsteed that we face the most serious charges against Newton. They were old friends, and Newton had received from Flamsteed a supply of observations of exceptional accuracy which had furnished the experimental data for, and verifications of, his theories. In return, Flamsteed desired Newton's support in securing the publication of his remarkable series of observations. The latter, indeed, was instrumental in obtaining the patronage and financial support of the Prince Consort for the project, but when the conditions were drawn up, Flamsteed's wishes were flouted, his remuneration was pitiably inadequate, and Newton used his own enormous influence to ensure that the work appeared in the form he (Newton) desired. When, in disgust, Flamsteed wished himself to issue the remainder of the work, he tried to recover the unpublished documents from Newton, but "no considerations of right ever prevailed, and he never returned Flamsteed's property. . . . When the second edition of the *Principia* appeared, in 1713, his name was erased in nearly all the places where recognition for his great services had previously been made ".

Professor More differs from previous writers in assigning much greater importance to Newton's share in the politics of the period. He has unearthed documents which show that Newton's part, both as a member of the deputation to the High Court in the affair of Fr. Francis (the Benedictine whom James II ordered the university to admit as Master of Arts without the usual oaths) and, later, as a member of parliament for the university, was considerably greater that is customarily supposed. His friendship with Montague (Lord Halifax) is also used as evidence of his political importance. The discussion will be found in detail in Professor More's book, but it seems to me inconclusive. With the new light now possessed on his character, one is not surprised to find that he carried out the duties assigned to him with the care that he devoted to all his work, but that his influence on the events of the day was important, is, I think, unproved.

Let me now turn to another topic, that of Newton's supposed financial troubles. Brewster is responsible for the legend that his circumstances were at one time such that he found it difficult even to pay his subscriptions to the Royal Society, and for that reason wished to resign from it. The truth seems very different. Even in childhood, he can scarcely be regarded as poor. It is true that his mother's income from her Woolsthorpe estate was small, but two years after his birth she married again, her second husband being a clergyman with a *private* income of £500 a year, part at least of which she may be assumed to have inherited on his death in 1656. Later, when Newton was Lucasian Professor, his annual income was at least £200, which at that period must have ensured full satisfaction of his modest requirements. How, then, did the story of his pecuniary troubles arise ? The only relevant document is the letter to Oldenburg, written in the spring of 1673, in which he asks to be allowed to resign from the society. In a postscript he mentions that he is sending his dues, which were half a year in arrears. On this slender basis, Oldenburg assumed his resignation was due to difficulty in paying the dues, whereas it seems certain that the true reason was disgust with the controversies into which he had been drawn by his communications, and that the reason given in the body of the letter, namely inability to attend the meetings, was a subterfuge intended to conceal his dislike of controversy and his animosity to Hooke. Finally, there are the gibes thrown at the Government for their supposed belated and inappropriate generosity in calling Newton from his scientific work to take charge of the Mint. Actually, it is found that he

himself applied for the post, and used the influence of his political friends in order to obtain it. He had long been weary of his profound investigations into nature; indeed, from his letters it would seem that at all times he found mathematical work arid and, to a certain extent, distasteful, and only performed it under pressure from others. It was, then, with relief that he found himself free to devote himself to the chemical and theological speculations which he regarded as most worth while.

These are a few of the questions raised and answered by Professor More's biography. It is well printed, clearly written, and a notable addition to the literature of the history of science. Few misprints or errors of importance have been found. There are surprisingly inaccurate statements on attraction on pages 233 and 297, but the correct form appears on page 302. The revolution of 1688 is antedated by a year in more than one place, and the dates of the letters on pages 68 and 205 should be 1727/8 (not 1827/8) and 1668/9 (not 1688/9) respectively.

One of the greatest tributes that can be paid to any book is that it makes one wish for more. In this case, one wishes for a complete edition of Newton's work and correspondence, worthy to take its place beside the magnificent editions of Galileo and Huygens produced in Italy and Holland. Is it not possible for the scientific societies of England to unite in the production of such an edition as the best possible celebration of the approaching tercentenary of Newton's birth?

C. W. GILHAM.

SIR ISAAC NEWTON'S **Mathematical Principles of Natural Philosophy** and his **System of the World**, translated into English by ANDREW MOTTE in 1729. The translations revised and supplied with an historical and explanatory appendix by FLORIAN CAJORI. Pp. xxxvi, 680. 35s. 1934. (Cambridge University Press for University of California Press)

The first translation of any portion of Newton's *Principia* into any language was Andrew Motte's translation of the whole work, which was brought out by his brother Benjamin, the publisher of *Gulliver's Travels*, in 1729, together with an essay by J. Machin on the motion of the moon. In 1777 was published the first volume of a translation and commentary by Robert Thorp *; no more volumes appeared, and in 1802 a reprint in which the work was described not as the first volume of a translation but as a translation of Book I made confession by this change to an abandoned enterprise. In the nineteenth century the sections which for many years formed the subject of "Newton" in the Tripos met with the attention natural to an examination text, and translations of these sections were expounded by a long succession of teachers. But the first translator has had no rival.

The next † appearance of Motte's translation was in 1803 under the care of W. Davis. The edition included Machin's essay, and added a life of Newton by the editor, a translation of Newton's *De Mundi Systemate*, and a comment on and a defence of the *Principia* by W. Emerson reprinted from a small

* The addition of "e" to the name in Gray's *Bibliography* (item 28 and index), 2nd ed., 1907, is a mistake, while in Zeitlinger's article in the M.A. commemorative volume, *Isaac Newton* 1642-1727, 1927, the name of Charles Thorp occurs in a sentence which obviously was superseded and should have been cancelled. Gray and Zeitlinger are the authorities for dates which I have not been able to confirm.

† The *D.N.B.* mentions a reprint of 1732, but no such reprint is known at Trinity or recorded by Gray or Zeitlinger.

volume published in 1770 which contained also defences of Newton's *Opticks* and *Chronology*. Let the editor speak for himself:

> The inconvenience arising from the great scarcity of former editions of Sir Isaac Newton's PRINCIPIA, and SYSTEM OF THE WORLD, added to the exorbitant prices charged for them when to be met with, determined the Editor to undertake a New Edition of those Works; and he is impressed with confidence, that no other apology will be thought necessary, at a time when Mathematics is become a fashionable science, and is looked upon as a necessary acquisition in the polite world.
>
> In compliance with the solicitations of several respectable Mathematicians, to this Edition is added Mr. W. Emerson's much admired COMMENT, and DEFENCE of Sir Isaac Newton's PRINCIPIA, thereby rendering the Work more easy of comprehension to students, and others, not fully acquainted with the higher branches of Mathematics; and the Editor is not without hope that the whole will be found generally correct: PERFECTION he has not yet thought of aspiring to.

With a reprint of Davis's edition in 1819, the story of Motte's translation comes to an end, as far as this country is concerned, to be resumed in America, where what was virtually the same edition of the *Principles* and the *System* with a life by N. W. Chittenden was issued by different publishers in 1848 and 1850. Now we welcome what must surely remain for a hundred years the authoritative edition, the translations of the *Principia* and the *De Mundi Systemate* revised and annotated by Florian Cajori, a mathematician almost uniquely qualified to present Newton to us again. Cajori died in 1930, but the manuscript had already been sent to the University of California Press, and we have therefore the satisfaction of knowing that text and notes are complete. There was no preface, and Prof. R. T. Crawford, who accepted from the Press the task of supervising the production, has wisely refrained from guessing at what Cajori might have said, omitting even acknowledgments since he could not be sure that they would be adequate.

Unlike the translation of the *Principia*, that of the *De Mundi Systemate* which has always been associated with it was published anonymously. It appeared first in 1728, and there were editions in 1731 and in 1737, but not again until 1803; Davis, it may be observed, attached no name to it. That it was by Motte has long been supposed; the edition of 1737 was published by Benjamin Motte, and verbal resemblances to the rendering of the *Principia* are so close that in Cajori's words it is "virtually certain that both books are put into English by the same translator", but Cajori is the first editor to show the courage of his conviction on the title-page. He offers no conjecture as to why a translator might wish to be unknown in the one case but not in the other. It will be remembered that although the *De Mundi Systemate* is undoubtedly authentic, it was actually a discarded first draft of the third book of the *Principia*; Newton attempted to write this book in a popular manner, but he regarded the attempt as unsuccessful; he did not destroy the manuscript, but he did not authorise publication, and it was not until after his death that the work appeared. Even then, the first edition of 1728 lapsed at once into obscurity: Castiglione in 1744 reprinted the edition of 1731 without mention of an earlier edition, and asserted that the translation preceded the original, and Gray has only the later date. In fact the circumstances were so suspicious that De Morgan, who had not heard of the Latin edition of 1728 and did not know that the manuscript in Latin was still in existence, justified scepticism—"I think I have a right to one little paradox of my own; I greatly doubt that Newton wrote this book"—by saying in the *Budget* (p. 83), "It is very possible that some observant turnpenny might

construct such a treatise as this from the third book, that it might be ready for publication the moment Newton could not disown it." * The most De Morgan will admit is that perhaps the translation was made from a mangled copy of a preserved manuscript, clandestinely taken and interpolated †, and when we find Gray (*Bibliography*, item 31), in comparing the second edition of the translation with the first, noting evidence that the translation of 1728 was made from a Latin manuscript which slightly varied from the one used in printing the standard text of 1731, we are reminded of De Morgan's words and suspect that if Motte did not attach his name to his work it was because the production of the *System* in 1728 was a transaction in some way discreditable.

In revising Motte's translations, Cajori has been careful not to modernise ideas or proofs, nor need the English reader be afraid of finding American spelling. All that Cajori has done, except when there were obscurities of language to remove, is to make such changes of terminology that the impact on the student of to-day is the same as the impact of the original on a reader two hundred years ago. One example will suffice: instead of "in a subduplicate ratio of the body S to the sum of the bodies $S+P$", a phrase which was immediately intelligible when it was written but which is unfamiliar now, we read simply "as $\sqrt{S}$ to $\sqrt{(S+P)}$".

The Appendix, which is Cajori's most personal contribution to this edition, seems short if we think of the intolerable proportion of sack to sustenance to which commentators are prone. But Cajori offers us no sack. There are notes, clearly written and thoroughly documented ‡, on such subjects as the nature of gravity, absolute motion, action at a distance, fixed infinitesimals, and causality, as well as on textual details. The one blemish on this magnificent edition, apart from a frequent use of *o* for 0 and some erratic printing on p. 666, is that the contents are neither tabulated in detail nor indexed. This is serious with regard to Book III of the *Principles* and to the *System*, and even more serious with regard to the Appendix; there is no means of knowing what the volume contains except by turning over the pages. To introduce headings into Book III would have been to tamper with the text, but in the *System* and the Appendix the headings already exist, and to have expanded the Table of Contents to include them would have added greatly to the value of the book.

To the *Principia* Halley prefixed an ode. Neither Motte nor Thorp ventured to translate the poem; Motte omits it, and Thorp reprints it in its Latin form. A version in rhymed couplets was given in Martin's *General Magazine* in 1755, but to modern ears this lacks entirely the dignity of the original, and it has no claim to be regarded as part of a standard translation §. For the new edition a translation into blank verse has been given by Prof. L. J. Richardson of California.

* The subsequent appearance of a Latin version has no bearing on the conjecture. Once accepted as authentic, the work, if originally in English, would demand, like Wallis' *Algebra* and Newton's own *Opticks*, translation into the universal language. In the case of Newton's *Method of Fluxions*, English translations from the manuscript were published in book form in 1736 and 1737, but the Latin original appeared for the first time in Castiglione's edition of Newton's works.

† Not "interpreted", as in Smith's edition of the *Budget*, i, p. 140.

‡ For the sake of completeness, two dates may be added: p. 646, Chasles, *Aperçu historique*, 1837—later editions are reprints; p. 656, M. Cantor, *Geschichte*, vol. 3, ed. 2, 1901.

§ The verses in Pemberton's *View of Newton's Philosophy*, 1728, are in English, but have no connection with Halley's ode.

Hampered by a language that is not to him the familiar instrument it was to his predecessors and by terms that are quaint even in translation, unable to distinguish for himself between what was already imperishable and what has been transformed almost beyond recognition before being absorbed into the living body of truth, the mathematical student no longer reads the *Principia*. Cajori has brought the *Principia* back within our reach, and made it possible for us all to understand why it can be said of Newton, in Richardson's rendering of Halley's line, that

Nearer the gods no mortal may approach.

E. H. N.

Numerical Studies in Differential Equations. I. By H. Levy and E. A. Baggott. Pp. viii, 238. 12s. 6d. 1934. (Watts)

The labour that must be put into a book of this kind is enormous, and that is perhaps why in this case the writers have been almost incredibly careless except in their arithmetic. I suspect that anyone who is capable of such constructions as " Using this figure, any gradient is readily transferred . . . ." (p. 21) and " An alternative method . . . is given . . . before proceeding. . . ." (p. 36) is incapable of seeing that there is anything wrong with them, but who could be blind to " In changing the axes . . . it is best to arrange that the average slope of the isoclinals to be as steep as possible over the whole range, rather than that they shall be steepest at one end of the range " (p. 41) ? Section 17·2 is headed 73·2, the reference in the footnote on p. 230 to " paraparaph (*sic*) 18·1 " is doubly wrong, the punctuation is unsystematic, Example 2 on p. 7 is barely intelligible, the curves in Fig. 2 on p. 37 should not go through the origin, erratic brackets make a sad mess of formulae on p. 74, while an unqualified " up to " means " up to but not including " on p. 74 and " up to and including " on p. 81. On p. 47 the value of one function only is described as the solution of a pair of simultaneous equations, on p. 72 a series of fractional powers is called a power series, on pp. 183, 184 functions are called fundamental solutions or integrals of one equation because they satisfy another equation, and on p. 207 the description of a bounded function is " V is finite, however large x is " ; the notation $f(x)\to g(x)$ is used as a matter of course to express an asymptotic relation between two functions. In a footnote on p. 84, it is said that certain series used in Frobenius' process must be uniformly convergent in order that differentiations indicated may be valid : (i) it is for integration, not for differentiation, that uniformity is in general the simplest sufficient condition ; (ii) the differentiations specified are successive differentiations with respect to x, of a series of the form $x^{a}(c_0+c_1x+c_2x^2+\ldots)$, and these differentiations are valid within the circle of convergence of the power series $c_0+c_1x+c_2x^2+\ldots$ irrespectively of the dependence of $c_0, c_1, c_2, \ldots$ on the parameter a ; (iii) the operation for which justification is a serious matter is differentiation with respect to a, and this is presently performed without comment ; that this differentiation is valid is a peculiarity due to the manner in which x is involved and is not to be inferred from the uniformity in a alone, although this is in the circumstances a sufficient condition.

The opening sections of the book, on the isoclinals and integral curves of an equation of the first order, are hard to read because the exposition never recovers from a false start : " If the equation $\phi(x, y, p)=0$ is algebraic in p of order n, then at any point (x, y) there are n values of the slope, and n branches of the integral curves pass through that point " (p. 6). But the subject is real equations, the number of values of the slope at an ordinary point may fall short of n by any even number, and the various sections of the p-discriminant

are in general precisely the frontiers separating regions which differ in respect of the number of integral branches through any point.

The use of Picard's sequence

$$y_{n+1}=y_0+\int_{x_0}^{x} f(x, y_n)\,dx$$

for the solution of the equation $p=f(x, y)$ is described as graphical integration or numerical integration according as the evaluation of the definite integral is supposed to be mechanical or arithmetical; the repetition of the discussion should drive home the principle. The transition from the development of a solution in a Taylor series, which is an obvious alternative to Picard's method, to the method of Frobenius, is made without the word of explanation that would help the learner to grasp that he has suddenly begun to deal with isolated singularities of his equation. It may indeed be asked why the purely algebraic discussion of this method is so pertinent to the subject of numerical integration that a reference to some textbook would not have been satisfactory. This question recurs when presently the standard elementary theorems on linear equations of the second order are reproduced. Do the authors regard their book as a practical supplement for students who have made the usual theoretical acquaintance with differential equations, or have they in mind students who will learn nothing of the subject elsewhere? The title is the clearest disclaimer to any completeness, but the authors have not taken advantage of their freedom.

The kernel of the book is an account of different methods of tabulating a solution of an equation or of a system of equations over a small initial range and of extending a solution once it has been begun. When all criticism of details and elaborations has been made, it can still be said that this kernel is sound. The explanations are clear and the comparisons are fair. It may be doubted whether the last word has been said on forward integration; essentially this is a problem of extrapolation, and improvements may be brought about either by an application of unformulating processes which take advantage of the fact that derivatives as well as functional values are known, or even by some radical change comparable with the use of reciprocal differences. Authors however must not speculate as freely as the irresponsible reviewer: their business is to report on methods that have been tested.

Interesting paragraphs deal with the roots of solutions, as indicated roughly by comparison with equations whose solutions are known; it is of course impossible to approximate in this way with indefinitely increasing accuracy, but even if the calculation must ultimately be performed by forward integration and inverse interpolation, a preliminary estimate of the range which will have to be covered is essential. The volume concludes with discussions of several problems in which a parameter enters into the differential equation; the importance of such problems in physics and astronomy is well known.

A second volume, which is to be concerned principally with partial differential equations, will be awaited with interest. E. H. N.

Alte Probleme—Neue Lösungen in den Exakten Wissenschaften. Fünf Wiener Vorträge (Zweiter Zyklus). Pp. iv, 122. RM. 3.60. 1934. (Deuticke, Leipzig and Vienna)

It was inevitable that an attempt should be made to repeat the success of the course of popular lectures on crisis and reconstruction in science given in Vienna last year (see *Gazette* XVII, 330; Dec. 1933), and this booklet reports a second course: Prof. Menger on the quadrature of the circle, Prof. Thirring on extra-terrestrial flight, Prof. Mark on transmutation of the elements, Prof. Scheminzky on artificial generation of life, and the late Prof. Hahn on the

existence of the infinite. The subjects are put in the form of questions, and Prof. Menger finds a common thread, though admittedly a slender one, in the observation that in each case the meaning of the question has changed since men first asked it. This is most striking in the last lecture, where what is at issue is not the nature of the infinite but the nature of the existence of mathematical objects.

The individual lectures are full of interest. Prof. Menger, for example, speaks of a paradox and its consequences: Why must the criminals in detective stories take so much trouble about the disposal of their victims, when anyone who has learnt dissection from Zermelo and Hausdorff can tuck away a corpse however large in his smallest pocket ? Prof. Hahn is at his brilliant best, learned and lucid, witty and wise, ranging from Plato and Aristotle to Russell and Einstein. A pathetic interest attaches to this discourse, the last he was to deliver to a public audience. A man whom civilization should have cherished, he died unhappy, but how deeply admired and loved, Prof. Menger's glowing tribute shows. E. H. N.

The Differential Invariants of Generalized Spaces. By T. Y. Thomas. Pp. x, 241. 21s. 1934. (Cambridge)

The geometries which were chiefly studied up to the beginning of this century are dominated by the Euclidean idea of space, namely something in which bodies exist and move. These are the geometries covered by Klein's Erlanger Program. They are direct generalizations of Euclidean geometry, the Euclidean idea of movement, or one of its generalizations, being expressed mathematically by means of a given group of transformations. But according to the general theory of Relativity physical space-time can best be represented as a space of the kind conceived by Riemann, namely as something with an intrinsic structure. Thus Relativity stimulated mathematicians to the study of Riemannian geometry and of the other kinds of geometry discussed in this book.

During the last fifteen years T. Y. Thomas has played a leading part in building up the subject on the analytical side, and the developments described here are, for the greater part, originally due to Thomas himself.

The various kinds of space to be considered are introduced in the first chapter. An interesting novelty is the discussion of invariants under transformations other than coordinate transformations before a discussion of the latter. Thus affine connections are introduced first (in terms of displacements, the definition of a vector being pre-supposed), and, after a section on paths, projective connections are found during a discussion of all the affine connections which define the same set of paths. The other kinds of geometry introduced in Chap. I are the Riemannian or metric, conformal, Weyl geometry and the generalized affine geometry in which there is distant parallelism.

In the next four chapters the spaces introduced in Chap. I are described in detail. Chapters II, III and IV are devoted to affine, projective and conformal invariants respectively, and Chap. V to affine and projective normal coordinates.

A great deal of the material in these five chapters appears in a book for the first time. This applies particularly to Chapters III and IV. The projective connections considered in the former are all of the type which define a system of paths, and this chapter, together with § 31 (Chap. V), gives an admirable account of the invariants arising in the projective geometry of paths. Chap. IV contains an account of the conformal covariant differentiation recently discovered by Thomas.

In describing the book these first five chapters, Chapters VIII and IX and

the first half of Chapter VII, will be grouped together. Chapters VIII and IX are on the equivalence problem and the reducibility of spaces, a space being reducible if, originally defined as belonging to one class, it is found to belong to a particular sub-class (*e.g.* if a Riemannian space is found to be Euclidean, or an affine space of paths Riemannian). The first half of Chapter VII contains a sketch of the Lie theory of groups and a section on the affine structure of the group space. For such a short account this could hardly be bettered.

These chapters are likely to be valuable and attractive to any reader. But they are particularly suitable for someone who, perhaps knowing no more than the elements of the tensor calculus, is anxious to learn enough to do useful research. The most striking feature is, I think, the way in which the different kinds of geometry (affine, projective, etc.) are laid out side by side for comparison. The introduction states that the book is exclusively analytical. This is true in the sense that the theorems are proved analytically. Also very little emphasis is laid on the processes of generalization implied by the terminology (*e.g.* from the classical to the generalized projective geometry). Nevertheless, there are many passages of geometrical interest, and these chapters are equally to be recommended to one who prefers the geometrical to the analytical point of view.

Chapters VI, VII and X provide for the solution of certain problems, typical of which is to determine whether or no a given set of functions can be the components of a normal tensor in normal coordinates.

In Chapter VII differential equations are set up which must be satisfied by a scalar differential invariant. Certain special cases are analysed in some detail.

Whereas the chapters previously discussed are of general interest, these are more for those who propose to specialize in "field equations", meaning differential equations in which the components of the fundamental invariant of a space enter as unknowns (*e.g.* those obtained by equating the curvature tensor to zero, or Einstein's field equations). The passages in question will serve as a valuable introduction to the author's recent work in this branch of the subject. J. H. C. W.

Variationsrechnung. I. By L. Koschmieder. Pp. 128. RM. 1.62. 1933. Sammlung Göschen, 1074. (W. de Gruyter, Berlin)

This little book (128 small pages) gives a good account of the first variation, the imbedding of a given extremal in a field and of conditions for an absolute minimum. Most of the work is in parametric form which is satisfactory from the geometrical and physical point of view at least. The Jacobi and Weierstrass conditions for a minimum are described in §§ 15 and 16 respectively. Each of these conditions is necessary, together they are sufficient. Perhaps it would have been better to introduce the Hilbert line integral at this point. As it is, the Weierstrass construction, described in § 14, is used to prove the sufficiency of the above conditions.

At the end of Chap. I it is shown how the locus of points which are conjugate to a given one envelops the family of extremals through the latter.

The third chapter describes the situation when the end points are free to move. The Hilbert line integral appears in the last section of this chapter.

The final chapter deals with isoperimetric problems, that is to say, the search for a minimum subject to certain extra conditions.

It will be seen that the scope of the book is considerable for its size. The author is all the more to be congratulated on account of the numerous examples which he works out to illustrate the principles under consideration.

J. H. C. W.

Lehrbuch der Topologie. By H. SEIFERT and W. THRELFALL. Pp. vii, 352. Geb. RM. 20. 1934. (Teubner)

In our conception of space a simple-minded idea of continuity comes before everything else. Ideas of distance, angle, straightness and so on are much more elaborate. Topology is the mathematical expression of the former. If a map were drawn on a rubber membrane its topological character would not be affected however drastically the membrane were distorted. Two islands would still be recognisable as such, but not square or circular countries.

This book opens with a chapter on the intuitive background of the subject. It is eminently successful in showing the extent to which topological problems pervade mathematics and how fascinating they are. There is no branch of mathematics which is more tantalizing to the imagination.

The systematic treatment of the subject begins with Chap. II. Spaces defined in terms of neighbourhoods are briefly discussed, and certain of the more fundamental theorems of point-set topology are proved. Then simplicial complexes are introduced and discussed in a general way. Chap. III contains an account of the homology and Betti groups, the latter being defined in terms of homology with division. Singular chains and deformations are discussed in Chap. IV, leading up to the proofs of invariance under homeomorphism.

There follows an interesting chapter on the topology of a complex in the neighbourhood of a point, and the remainder of the book (pp. 130-314) is either explicitly on manifolds or on matters which are chiefly interesting for their application to manifolds. There are good accounts of two and three-dimensional manifolds in Chaps. VI and IX respectively. In the former the polygonal representation of a surface is reduced to normal form by an elegant method similar to the one first used by H. R. Brahana. It is then an easy matter to read off the invariants of the homology group from the normal form. After chapters on the fundamental group and the covering complex of an n-dimensional complex, it is shown how to represent a three-dimensional manifold as the interior and boundary of a polyhedron (here there is no known method of reduction to normal form). The Heegaard diagram is described and the construction of three-dimensional manifolds out of knots. Sufficient examples are given to show how the theoretical passages can be applied to the study of particular manifolds.

The remainder of the book is devoted to n-dimensional manifolds (Poincaré duality, intersections, looping, etc.), continuous transformations (degree of a transformation, fixed point formulae, etc.) and additional theorems on groups. In the choice and presentation of material these chapters are up to the high standard of the previous ones. However, it seems a pity that Alexander's duality theorem was not included in the chapter on n-dimensional manifolds. The essence of the proof is stated in a brief appendix, but a few extra pages would surely have been well spent in a detailed discussion of this beautiful theorem.

In all, the material is presented clearly enough and with sufficient imagination to provide a first-class introduction to the subject. Moreover, the scope of the book is such that research workers will find it a stimulus and to many of them it will be a source of new knowledge. J. H. C. W.

Vorlesungen über die Theorie der Polyeder. By E. STEINITZ, edited by H. Rademacher. Pp. viii, 351. Geh. RM. 27; geb. RM. 28.80. Die Grundlehren der mathematischen Wissenschaften, 41. 1934. (J. Springer, Berlin)

Most people who write about polyhedra are concerned with special forms, such as regular or crystallographic polyhedra. Steinitz has deliberately kept

away from special figures, save as examples, since they have been fully treated in such works as Brückner's *Vielecke und Vielflache.*

Many results which one is tempted to take for granted are here proved, with admirable thoroughness and rigour. Subsequent authors will surely be grateful that they can quote such results, instead of either assuming them as "obvious" or trying to prove them for themselves.

The general theory of polyhedra—even of convex polyhedra—is rather a tantalizing subject. The natural question, "How many types of polyhedra have n vertices?", can be answered for small values of n, and anyone who is sufficiently industrious can take values a little higher than his predecessors; but very little is known when n is arbitrary.

Steinitz introduces polyhedra from many different aspects: metrical, combinatorial, topological, analytical, projective and axiomatic. Then, having laid the foundations, he is content to stop. This treatment causes a certain lack of unity in structure, and the reader must not expect to find any startling conclusions. Each general statement is amply illustrated by examples; this accounts for the book's great length, but makes it more readable than it could otherwise be.

Much of the latter part is concerned with proving in various ways the *Fundamental Theorem of Convex Types.* Steinitz defines a "K-polyhedron" as a Eulerian polyhedron with the property that whenever two faces have two vertices in common, the join of these vertices is an edge. Clearly every convex polyhedron is a K-polyhedron. The "Fundamental Theorem" states that every K-polyhedron is isomorphic with a convex polyhedron.

Although the book has only a paper cover, it is elegantly printed on good paper, and the 190 diagrams leave nothing to be desired. H. S. M. C.

Théorie de l'intégrale. By S. Saks. Pp. viii, 290. $4. 1933. Monografje Matematyczne, 2. (Warsaw)

The book starts with the theory of functions of an elementary figure. The main results of the theory are established, which allows the author to escape repeating the same argument at various occasions and which also brings unification in the methods used in the book.

Then follows a very thorough and complete development of the theory of the Lebesgue measure and of the Lebesgue, Denjoy and Perron integration, and of the mutual relation of these processes of integration (the Hake and Alexandroff-Looman theorems are given).

Apart from the pure theory of integration the following problems are considered:

(1) Area of a surface $z=f(x, y)$. The author goes as far as to give the Tonelli theorem.

(2) Properties of functions of a single variable with respect to differentiation. The main Denjoy results are fully represented.

(3) Differentiability of functions of two variables. The author gives an account of work of H. Rademacher, W. Stepanoff and U. S. Haslam-Jones.

The book is concluded by the chapter on Lebesgue integrals in abstract spaces and by a note by S. Banach on the Haar measure.

The book as a whole is planned remarkably well and also one feels that all the details have been thought over very carefully. The whole subject of the book is systematically developed on deep principles of the modern Analysis. From the point of view of the material represented in the book it is valuable both for an expert in the subject and for a beginner. For all that, the author has succeeded in making the book quite easy to read.

The book is a great event in the literature on modern Analysis and an excellent success for the collection to which it belongs (Mathematical Monographs, Warsaw). A. S. Besicovitch.

Foundations of the Theory of Algebraic Numbers. II. By H. Hancock. Pp. xxvi, 654. $8. 1932. (Macmillan)

Vol. I of this work was reviewed in the *Gazette* of October 1932.

Vol. II opens with three chapters (120 pages) on ideals, but without any numerical example of an ideal in a realm of any degree. The next three chapters deal with aspects of the work of Kronecker, Hurwitz and Hensel, whose disciple the author is. Chap. VII gives at least two theories of the units in a number-realm; here too something is lacking in that no example of any unit in an actual realm is given. Next we have five chapters on Minkowski's theorem, the Class-Number, Order-Moduls, composite forms, and relative-realms. Chaps. XIII-XV contain an account of Galois' Theory of Equations, a subject that seems foreign to the rest of the book. And finally there is a chapter on Hensel's p-adic numbers.

The second volume of Prof. Hancock's book is more of the nature of an encyclopaedia than a textbook. Many of the theorems are given in duplicate, but, to the average reader, one proof followed by a numerical example would be more stimulating. As in Vol. I this redundancy makes it difficult for a reader to pick out the essentials, and almost impossible for a beginner to build up the theory in any correct sequence. In both volumes the author gives the impression of only having studied the subject (apart from the work of Hensel and Hurwitz) up to the stage at which it stood thirty years ago. Galois' theory of equations, for example, has enjoyed several more recent developments. The few examples given hardly test the reader's manipulative skill, and are not satisfying. We can recommend the book to those desiring to possess an encyclopaedic treatise on the whole subject, but hardly to a reader desiring to know what the subject is all about. W. E. H. B.

Mind and Nature. By H. Weyl. Pp. viii, 100. 6s. 6d. 1934. (University of Pennsylvania Press, Philadelphia; Oxford University Press)

What is Truth? Or rather, what constitutes a "true" physical theory? Professor Weyl illustrates his answer by classical electromagnetic theory. Consider a number of charged particles in otherwise empty space. Then, apart from retarded values, the positions and velocities of the particles at time t determine the electromagnetic field. By the laws of this field the forces exerted on the charges can be calculated. Hence, by the laws of mechanics, the accelerations of the charges at time t are determined, and so by a process of integration their complete motion is calculable. Experiments can be made to test this theory only as a connected whole. If the calculated paths of the particles agree with observation, then the theory is "true" in the only sense that matters for physics. It has no meaning to isolate one "law" from the rest of the theory and ask whether or not that law is "true". One important consequence is that physical truth is not necessarily unique.

How then does physical theory develop? Professor Weyl points out that "one looks for the least possible change in the historically developed theory that may account for the new facts". It is only the approximate truth of existing theory that provides a starting point for, and allows one to attach a meaning to, any refinements brought in on account of new discoveries.

These remarks of the author's show, firstly, the essential unity of physics, and, secondly, the essential value of all past work. Analogies are always dangerous, and Professor Weyl is not guilty of this one; but one can perhaps compare physics with a tree. If the tree is growing properly, it is a better

tree this year than it was last year. Yet its form this year is dependent upon its form last year, and of course on all its previous history. Similarly the form possessed by physical theory to-day depends vitally upon all that has gone before.

Professor Weyl's comments upon these topics are scarcely more than asides in this slender volume crammed with profound thought. It consists of his five Cooper Foundation lectures at Swarthmore College for 1933. One can only say that if the Swarthmore collegians were able to assimilate this amount of material in the space of a mere five hours, then Professor Weyl must have had an audience of supermen! But for us ordinary mortals it is a great boon to have the lectures on paper and to be able to ponder them for as many hours as we please.

The several lectures deal with Sense Perception, World and Consciousness, Scientific Concepts and Theories, Relativity, Quantum Physics. In his progress through these subjects the author illustrates the general argument by discussing the theory of colour vision, the problem of determining a quantity "more exactly than its distinctness in sense perception allows", the philosophy of the mathematical method in physics from the standpoint of the "correspondence theory" of knowledge, the observation of the stars of a constellation as an example of the subject-object relation in relativity theory, the additional insight into the subject-object relation arrived at by quantum physics, and many other problems small and great. To read the book is to witness a master mind take stock of our knowledge of Nature, and indicate to us how all the different parts and aspects can be fitted together into a comprehensive unity.

One closes the book with the renewed conviction that there ought to be a universal rule that only those who have themselves made first-class contributions to mathematics and physics should be allowed to philosophize about them! This would save us from a lot of talk that leads nowhere, and leave the field open for the truly profitable discussions of writers like Professor Weyl.

W. H. McCrea.

Bessel Functions for Engineers. By N. W. McLachlan. Pp. xii, 192. 15s. 1934. Oxford Engineering Science Series. (Clarendon Press)

As the title suggests, this book is an attempt to present the subject in a form suitable for engineers. The author states that the treatment is "simple yet rigorous enough for engineers". Some of the simplicity is certainly due to the fact that quite a number of formulae are quoted without proof. Perhaps it would have been more helpful to engineers if the author had made it clearer at times that he was making assertions which he hoped his readers would be prepared to believe.

The book has been written mainly with a view to the applications of Bessel functions to acoustical and electrical engineering, and many problems connected with these subjects are considered in various parts of the book. The preliminary consideration of functions of zero order in Chapter I is a useful feature for the class of students for which the book is intended. For example, there are graphs of $J_0(z)$ and $Y_0(z)$, and the main features of the functions are pointed out.

Naturally the work is not an encyclopaedic one; the needs of the engineer have been kept in mind almost continually, and yet there are occasionally strange departures from this point of view. For example, on page 64, the formula

$$J_{n+\frac{1}{2}}(z)=\frac{2(\frac{1}{2}z)^{n+\frac{1}{2}}}{\sqrt{\pi}\,.\,n!}\left\{\left(1+\frac{d^2}{dz^2}\right)^n\frac{\sin z}{z}\right\}$$

is quoted without proof, together with the similar formula for $Y_{n+\frac{1}{2}}(z)$. One wonders whether these formulae are of any use to the engineer, compared with the special cases of the asymptotic series for $J_\nu(z)$ and $Y_\nu(z)$, which are also quoted without proof.

A curious error occurs on page 48, where it is stated that

$$\int_0^z J_n(z)\,dz$$

converges only when $n+1>0$, n being an integer, positive or negative; and yet the author has several times pointed out the connection between $J_n(z)$ and $J_{-n}(z)$. The formulae concerned are actually true when n is a negative even integer, and need only slight modification when n is a negative odd integer.

The statement on page 2 that the publication of Fourier's *Analytical Theory of Heat* was delayed for twelve years " for fear that it should adversely affect the prestige of the powers that were " is interesting, but is it true? The author's industry in inserting strange initials to various mathematicians shows signs of considerable research. The initials given to Gauss are certainly unusual.

The book will no doubt be welcomed by engineers who need Bessel functions in their work, but it is certainly more suitable for engineers than for mathematicians. W. N. B.

Elementary Quantum Mechanics. By R. W. Gurney. Pp. vii, 160. 8s. 6d. 1934. (Cambridge)

This book provides a clear account of the main ideas of quantum mechanics and should prove of great value to students of experimental physics. The orbit, so useful and so easy to visualise in the days of the " classical quantum theory " is replaced by the ψ-pattern, which, by means of profuse illustrations, in particular the beautiful plate opposite page 14 reproduced from the *Physical Review*, is made to seem as concrete an idea as the orbit. A clear account of the one-electron problem, in particular of the hydrogen atom, is given and this is followed by an outline of the main problems of physics and chemistry to which quantum mechanics has been applied, the formation of molecules, valence bonds and properties of metals and crystals. The last two chapters are on perturbation theory and they form a valuable introduction to the original papers and the large body of research which makes use of perturbation methods. A short bibliography is given; it would be absurd in this subject to expect anything approaching completeness, but it is surprising to find the work of Slater and of Hund omitted from the references to work on homopolar molecules and valency.

The historical development of the theory is not of first importance in a work of this kind. Nevertheless, it is unfortunate that the theory is so emphatically made to appear compounded of a number of lucky guesses; for example, in a remark on the formula for the de Broglie wave-length associated with a particle, " This expression was derived only by analogy; like most of the ideas of quantum mechanics it was a conjecture ", and in the following chapter, " Schrödinger hit on the idea— ". The particular analogy between the motion of a particle in a field of force and the propagation of a wave in a heterogeneous medium had been known for nearly a century when de Broglie first used it in this field, although his work appears to have been independent of Hamilton's. It is at least arguable that the quantum theory, in spite of the vicissitudes it has undergone in its short lifetime, is, in the main stream of its development, no more " conjectural " than other physical theories. It is precisely the removal of the mass of arbitrary assumptions which had begun

to impede the course of the older quantum theory which has made quantum mechanics so powerful in coordinating the large range of phenomena with which this book deals. **B. Swirles.**

Electromagnetism. By H. M. Macdonald. Pp. xv, 178. 12s. 6d. 1934. (Bell)

This book gives a connected account of the "classical" electromagnetic theory, deduced from the laws of Ampère and Faraday, along with Fresnel's law of transversality. These, together with the assumption that material media can be represented by a distribution of electric and magnetic current throughout the space they occupy, are found to be a sufficient basis for the theory, which is then given in eight chapters. Particular problems are worked out by way of illustration. The whole is clearly written, although the treatment is concise, and demands considerable mathematical equipment on the part of the reader. **W. G. B.**

Five Place Table of Natural Trigonometric Functions to Hundredths of a Degree. Compiled by Amelia de Lella. Pp. 50. 6s. 1934. (John Wiley and Sons, New York; Chapman and Hall, London)

A few years ago we took the house of Wiley to task for reproducing, in a table by H. C. Ives, figures previously published in Brandenburg's *Siebenstellige trigonometrische Tafel.* The house has now offended again by publishing, in the book before us, figures that appeared in Lohse's *Tafeln für numerisches Rechnen mit Maschinen* (Leipzig, Engelmann, 1909). Lohse's tables, which give five-figure natural values of the six trigonometrical functions at intervals of 0°·01, with their differences, are a model of accuracy, good arrangement and good printing; De Lella's tables fail in all these respects.

The tables have been prepared from the *Trigonometria Britannica* (mis-spelt *Brittanica* in the Preface), but the compiler has introduced three gross errors, while in thirteen cases the last decimal is too high by a unit, in each instance where the rejected figures lie between 495 ... and 499 ... .

The following is a specimen of the arrangement:

12°

		·00	·01	......	·09			
12·0°	sin	·20791	·20808		·20945	·20962	cos	
	cos	·97815	·97811		·97782	·97778	sin	
	tan	·21256	·21274		·21420	·21438	cot	
	cot	4·7046	4·7006		4·6686	4·6646	tan	·9
·1	sin	·20962	·20979		·21115	·21132	cos	
	cos	·97778	·97775		·97745	·97742	sin	
	tan	·21438	·21456		·21602	·21621	cot	
	cot	4·6646	4·6606		4·6291	4·6252	tan	·8
		...	...		...	...		
			·09		·01	·00		

77°

A cruder arrangement would be difficult to conceive. Successive values are in rows instead of being in columns—an arrangement that a good tabulator adopts only under extreme pressure. The medley of sines, cosines, tangents and cotangents precludes any possibility of learning to find one's way naturally about the table; any desired value must be traced laboriously by searching for the incongruously scattered components of its argument.

The copy has been prepared on an ordinary typewriter and indifferently reproduced by photo-lithography. While this process is suitable for the production of a few copies for private use, or for a work that would otherwise be unremunerative, it is hardly sufficiently dignified for a table that should anticipate a reasonable sale and is priced at 6s. The effect is certainly not attractive.

We are told that the " table has been compiled in response to a frequently expressed desire on the part of engineers for a five-place table of natural trigonometric functions of angles expressed in degrees and hundredths of a degree ", and are glad to note that the engineer is now following the lead of the astronomer and discarding the sexagesimal division of the degree in favour of the decimal division. The desire for natural functions must also be taken to imply that logarithms are yielding to calculating machines in one of their last strongholds.

L. J. C.

Mathematical Excursions. By Helen Abbot Merrill. Pp. xi, 145. \$2.00. 1934. (Bruce Humphries, Boston)

During the last few years a great many writers have endeavoured to introduce the non-specialist to scientific knowledge of the most abstruse nature. There have been popular guides to Relativity while the elementary side-paths of Mathematics have been neglected except for those few mathematicians who have had leisure enough to follow them. This book of Professor Merrill's seeks to make known to those who have the very minimum of mathematical equipment some very interesting facts, which might be described as by-products of the subject. The most interesting pages, and by far the best, are those dealing with arithmetical peculiarities or what the author describes as " oddities of numbers ", and if every schoolboy and schoolgirl could be persuaded to read these and to attempt all the problems that are given, they would certainly enjoy their formal lessons more than before doing so. It is amusing to notice how pieces of " examination book-work " are sandwiched in between some of the " oddities ", almost as though the former were unpleasant medicine which requires sweets before and after. The weakest pages are those dealing with elementary inversion; in fact there does not seem much point in giving what may be found in any textbook on Modern Geometry, and it is certainly not a " side-path ". However, it is impossible to write a book of this kind that would not be subject to some criticism. While the book is primarily for the non-specialist yet teachers may learn from it some particularly effective ways of demonstrating such algebraic formulae as those for Σn, Σn^2 and Σn^3; in short, it might be described as " Rouse Ball's Lectures at the Royal Institution ". The last chapter mentions certain theorems in the Theory of Numbers which still remain to be proved, and also the surprising fact that efforts are still being made to find an exact rational value of π.

Alex. Inglis.

A Shorter Trigonometry. By W. G. Borchardt and A. D. Perrott. Pp. viii, 238, xxxii, xxxi. 4s.; without tables (pp. xxxii), 3s. 6d. 1934. (Bell)

We can strongly recommend this cheap and excellent book to all those who have an eye on the Matriculation and like examinations. Each chapter ends with a large number of drill exercises and problems, which are not too difficult for the class of students for which they are intended. So both students and teacher may open the book anywhere at random without the fear of dropping on something that could not be of service.

In the first hundred pages, the treatment is mainly numerical and ends with

the solution of triangles, and height and distance problems in three dimensions. The analytical part of trigonometry is then gradually introduced—compound angles, products and sums, general values, the properties of triangles, etc., being treated in a workmanlike manner.

The authors do, however, make a mistake, common but fundamental, when they identify a magnitude (length) with numerical quantity. They say (p. 86)

$$OM = OP \cos A = \cos A \,(\text{since } OP = OQ = 1)$$

and so use OP to denote both a length and a measure of a length and accordingly obscure the concept of $\cos A$. In the usual circle, OM is certainly proportional to $\cos A$, but the constant of proportionality is not an abstract number—it is a length.

Generalised ratios are introduced as operators which make correspondence between polar and rectangular coordinates—not without a certain amount of blurring, however. A point in the second quadrant should be specified by $(-c, +d)$ and a point in the first quadrant by $(+a, +b)$, and not by (a, b), a, b, c, and d being signless numbers. And then

$$\tan \theta = y/x = +d/-c = \text{negative quantity},$$
$$\tan \theta = y/x = +b/+a = \text{positive quantity},$$

refer respectively to obtuse and acute angles, x and y denoting directed numbers. Nothing but confusion will result if OM be used to denote first a signless length and then a directed length, or if x be used to denote first a and then $+a$. Incidentally, there is no need to restrict the generalised polar coordinate r to be $+\rho$ (ρ being signless) any more than there is to restrict x to be $+a$. Indeed in many parts of Mathematics and Science we are forced to admit the possibility $r = -\rho$.

The authors have been well served by the printers except in one respect—a medley of heavy type has often the effect of stressing the least important part of the work. V. N.

Solid Mensuration. By W. F. Kern and J. R. Bland. Pp. viii, 73. 7s. 6d. 1934. (John Wiley and Sons, N.Y.; Chapman & Hall)

This book covers the mensuration of the prism, pyramid, cylinder, cone and sphere and is of about school certificate standard.

Each solid is defined, its properties are stated and the formulae relating to it are given. And then follows a set of examples on the mensuration of objects of common experience. The problems are spread over a wide range and will, as the authors claim, give the student ample opportunity of improving his space intuition. No proofs are given as the course is intended to be taken concurrently with or after an organised course in solid geometry. But the price makes it difficult to know to whom to recommend the book. Students in this country are not likely to be inveigled into paying 7s. 6d. for a set of examples in mensuration.

We wish writers of books on mensuration would agree among themselves on what is to be the definition of a prismoid or prismatoid. The authors define it as a polyhedron all of whose vertices lie in two parallel planes. We mention this because Perry in defining a prismoid writes: "Let there be two closed curves or irregular polygons on parallel planes, and let these be the ends of the prismoid. Imagine these joined by a surface which is made up of parts of cones or planes (a developable surface it is called). This is the most general definition of a prismoid that I can think of." And his illustrative example concerns a barrel!

The tendency seems to be to call by the name prismoid any solid whose volume is given by Simpson's formula, $V = \frac{h}{6}(A + B + 4M)$, the so-called prismoidal formula. One might just as well call every solid a cube, because the volume of any solid can be specified by L^3, L being a length. V. N.

Examples in Elementary Statics and Dynamics. By R. C. Fawdry. With Answers. Pp. vii, 145, xviii. 3s. 6d. 1934. (Bell)

We have here the examples from the author's *Statics, Pt. I* and *Dynamics, Pt. I* in one volume, with the addition of some miscellaneous questions. The exercises in this collection and in the two textbooks are numbered in exactly the same way (with Statics first), so that they can easily be used together. The great majority of the examples are numerical and quite simple, the course being intended for the non-specialist in mathematics, while the additional questions are slightly harder and of the kind often set in examinations. The course covers the usual elementary work, up to and including, in Statics, Friction, Three-Force problems and Centre of Gravity; and in Dynamics, Relative Velocity, Projectiles and Motion in a Circle, but not Simple Harmonic Motion. J. W. H.

Elementary Algebra. III. By A. W. Siddons and C. T. Daltry. Pp. viii, 248, lxxv-cxiii. 3s. 6d.; without answers, 3s. 1934. (Cambridge)

This would appear to complete the revised version of Godfrey and Siddons' *Elementary Algebra.* The other parts have already been reviewed in the *Gazette* (XVII, December 1933, No. 226; XVIII, May 1934, No. 228). In general criticism there is nothing to add to these reviews. Part III has all the excellencies of Parts I and II. The authors have retained the exposition of Part II of Godfrey and Siddons, recognizing that these methods have stood the test of experience. In this volume then may be found the same treatment of variation, and the same introduction to series and to the Binomial theorem that were commendable features of the earlier work. But a chapter on Permutations and Combinations provides for a fuller treatment of the Binomial theorem. F. C. B.

Outline of the History of Mathematics. By R. C. Archibald. 2nd edition. Pp. i, 58. 50 cents; 10 or more copies, 40 cents each. 1934. (Mathematical Association of America, Oberlin, Ohio)

The first edition of Professor Archibald's masterly sketch of the history of mathematics was reviewed in the *Gazette* for December 1932 (XVI, p. 367). For the second edition, the author has corrected some minor errors, has added some notes to the list of books for further reading, and has supplied a new section on Babylonian mathematics. This new section is based on the truly astonishing discoveries of Otto Neugebauer, who appears to have demonstrated that the Babylonians of about 2000 B.C. were able to solve quadratic equations, to sum geometric progressions, and even to solve cubic and biquadratic equations. T. A. A. B.

An English Bibliography of Examinations. 1900-1932. By M. C. Champneys. Pp. xxiv, 141. 5s. 1934. (Macmillan)

" This Bibliography has been prepared for the International Institute Examinations Inquiry by Mrs. Mary C. Champneys under the direction of Sir Michael Sadler and Sir Philip Hartog. The main portion of the book consists of a bibliography of books and articles in periodicals published in Great Britain from 1900 to 1932. In addition there is a short list of publications bearing on the general history of education that have influenced the examination question from 1700 onwards, contributed by Sir Michael Sadler."

This quotation indicates very clearly the scope of the book. An examination of the main list makes it evident that the net has been widely and skilfully cast; it is a little more difficult to be certain that the meshes have performed their function successfully. A good selection of titles of *Gazette* articles is made, but there is one astounding omission—G. H. Hardy's Presidential Address, " The case against the Mathematical Tripos " (Vol. XIII, p. 61). T. A. A. B.

PRINTED IN GREAT BRITAIN BY ROBERT MACLEHOSE AND CO. LTD.
THE UNIVERSITY PRESS, GLASGOW

BOOKS RECEIVED FOR REVIEW.

R. C. Archibald. *Outline of the history of mathematics.* 2nd edition. Pp. i, 58. 50 cents ; 10 or more copies, 40 cents each. 1934. (Mathematical Association of America, Oberlin, Ohio)

P. B. Ballard and J. Brown. *The London Arithmetics. Second series. Pupil's book I, Pupil's book II.* Each, pp. 80. Paper, 10d. ; limp cloth, 1s. each. 1934. (University of London Press)

G. W. Brewster. *Trigonometry.* Pp. viii, 304. 5s. 1933. (Oundle School Bookshop)

G. W. Brewster. *Calculus and coordinate geometry.* Pp. vii, 326. 5s. 1934. (Oundle School Bookshop)

J. T. Brown. *The elements of analytical geometry. III. Conic sections.* Pp. 169-325, vii. 2s. 6d. 1934. (Macmillan)

A. De Lella. *Five Place Table of Natural Trigonometric Functions to Hundredths of a Degree.* 6s. 1934. (John Wiley and Sons, New York ; Chapman and Hall)

H. Levy and E. A. Baggott. *Numerical studies in differential equations. I.* Pp. viii, 238. 12s. 6d. 1934. (Watts)

A. E. E. McKenzie. *Hydrostatics and mechanics.* Pp. x, 272. 3s. 6d. 1934. (Cambridge)

N. W. McLachlan. *Bessel functions for engineers.* Pp. xii, 192. 15s. 1934. Oxford Engineering Science Series (Oxford)

G. S. Mahajani. *Lessons in elementary analysis.* 2nd edition. Pp. xii, 264. Rs. 5. 1934. (Aryabhushan Press, Poona)

H. E. Merrill. *Mathematical excursions.* Pp. xi, 145. $2.00. 1934. (Bruce Humphries, Boston)

L. T. More. *Isaac Newton : a biography.* Pp. xii, 675. 18s. 1934. (Charles Scribner's Sons)

M. Morse. *The Calculus of Variations in the Large.* Pp. ix, 368. $4.50. 1934. American Math. Soc. Colloquium Publications, 18. (American Mathematical Society)

K. Reinhardt. *Methodische Einführung in die höhere Mathematik.* Pp. iv, 270. Geb. RM. 14. 1934. (Teubner)

M. Roberts and E. R. Thomas. *Newton and the Origin of Colours.* Pp. viii, 133. 3s. 6d. 1934. Classics of Scientific Method. (Bell)

A. W. Siddons and C. T. Daltry. *Elementary Algebra. III.* Pp. viii, 369-615, lxxv-cxiii. 3s. 6d. ; without answers, 3s. 1934. (Cambridge)

D. E. Smith. *The Poetry of Mathematics, and other essays.* Pp. v, 91. Paper, 50 cents ; cloth, 75 cents. 1934. The *Scripta Mathematica* Library, 1. (*Scripta Mathematica*, New York)

J.-B. Tourriol. *Optique géométrique.* Pp. vi, 300. 35 fr. 1934. (Gauthier-Villars)

J. H. M. Wedderburn. *Lectures on matrices.* Pp. vii, 200. $3.00. 1934. American Math. Soc. Colloquium Publications, 17. (American Mathematical Society)

K. P. Williams. *The calculation of the orbits of asteroids and comets.* Pp. vii, 214. 15s. 1934. (Principia Press, Bloomington, Indiana ; Williams and Norgate)

A. Wisdom. *Century sum books. IV A, IV B.* Pp. 64 each. Paper, 9d., limp cloth, 10d. each. 1934. (University of London Press)

Alte Probleme—Neue Losungen in den exakten Wissenschaften. Fünf Wiener Vorträge : zweiter Zyklus. Pp. 122. M. 3.60. 1934. (Franz Deuticke, Vienna)

space. It was best taught between the ages of 8 and 12, for older children seemed to find difficulty with three-dimensional representation. The first aim should be to convince children of the inadequacy of their own attempts. Models should be freely handled and drawings made in simple oblique projection. Then the emphasis might pass to workshop applications, and plan and elevation should be introduced when it was useful for solving the problems arising from such work. It was less desirable to stress the nets of solids. Finally there came the discussion of simple exercises involving congruence, map sections, construction of curves—for example, for shaping the hull of a boat—interpenetrations, and exercises requiring isometric projections.

C. T. DALTRY, Hon. Sec.

NORTH EASTERN BRANCH.

REPORT FOR THE YEAR 1933.

FOUR meetings were held during the year.

The first was held on 18th February, in conjunction with the Mathematics Section of the Annual Conference of University and School Teachers of Schools taking the School Examinations of the University of Durham. Mr. E. H. Axton, M.A., Headmaster of the Pendower Central Commercial School, Newcastle-upon-Tyne, gave a paper on "Mathematics in Selective Central Schools".

On Friday, 24th March, Sir Westcott S. Abell, of Armstrong College, addressed the Branch on "Numerical Integration and Ship Calculations", in which he gave, amongst other interesting items, a demonstration of the use of the planimeter.

On Friday, 20th October, Mr. G. Manley, of the University of Durham, gave a paper on "The mathematical approach to the study of Climate", in which he said that the search for periodicity in climate has been unsuccessful and that he believes that it is of little value. Statistical correlation had been tried in the study of climatic problems but had little success, and he said that experience and judgment were qualities required before mathematical ability in approaching the study of climatology. He mentioned that the official weather forecasts were correct to about 77 per cent. in London and diminished to about 70 per cent. in the Western Highlands.

On Saturday, 25th November, the Branch was addressed by the President of the Association, Professor G. N. Watson, F.R.S., on "Reciprocal Functions". He defined two functions as reciprocal when an operation performed upon either gives the other; *e.g.* $O(z)=1/z$, $O(z)=a-z$. He gave an account of modern work on these functions and self-reciprocal functions by Hardy, Titchmarsh and himself.

J. W. BROOKS, Hon. Sec.

FOR SALE.

Offers requested for Nos. 84-220 of the *Mathematical Gazette*, in good condition. Apply to S. J. N. MacKinlay, Innisfallen, Cookstown, Ulster.

Mathematical Gazette, Vols. I-XVIII complete, and Index I-XV. Nos. 7-114 bound (with wrappers) in six volumes, half calf: rest, including Nos. 1-6, in parts as issued. £15. Apply to Dr. D. M. Wrinch, Lady Margaret Hall, Oxford.

Nos. 1-6 are now so scarce that a set including these numbers and ending in the middle of Vol. XV appeared in a recent bookseller's catalogue at £18.

LONDON BRANCH.

The opening meeting of the new session was held at Bedford College on 29th September, when Professor Hamley, of the Institute of Education, addressed an audience of about 100 members and visitors on " Research in the Teaching of Mathematics ". The speaker said that his general conclusion would be that, although a large number of research studies had been reported, there was little of permanent value in the results. On the positive side it could be said that we now knew a great deal about the relative difficulty of number combinations but we were still uncertain about methods of teaching. Some researches had given contradictory results, owing to unreliable statistical methods. In America much attention had been given to the analysis of objectives in the teaching of mathematics. This analysis was based on the needs of everyday life, the requirements of the sciences and technical work and on the philosophical foundations of mathematics. Attention had also been given to the technical processes of mathematics: for instance, those required in problem solving. It had been definitely proved that the urge for speed was detrimental to success, that the child did his best work when working just within his maximum rate. It had also been proved that children made better progress when they were permitted to look up and check their answers. An important aspect of the subject was the formulation of diagnostic tests to discover individual weaknesses. Much had been done on methods of class procedure. It had been proved, for example, that children did much better work, in some cases, when they were allowed to work in groups of twos or threes. When methods and processes were examined it was rarely possible to say that one method was definitely wrong or even inferior to another. A common source of inability to learn algebra or geometry was failure, on the part of the pupil, to see an expression or a diagram as a whole. It was possible to improve the perceptual " span " in seeing algebraic expressions and geometrical figures by practice. In all mathematical teaching it was desirable to stress the functional rather than the formal or computational aspect. Reference was made, in this regard, to the work of Klein and Nunn.

Professor Hamley discussed the work that needed to be done in analysing mathematical ability and in the study of teaching devices to produce effective results. He suggested that the Mathematical Association might produce some such collection of devices as the book recently issued by the Science Masters' Association. Among special problems were the following: How far should school mathematics be based on the concepts that were fundamental to higher mathematics? What was the nature of mathematical thinking and what were the factors involved? To what extent and under what conditions will the study of mathematics transfer to the problems of everyday life? How far is it possible to predict mathematical ability at an early age? Were there parts of the mathematical curriculum that could be omitted because they could be apprehended without teaching; were there parts that could be much more easily learned by being postponed a year or more? Was there not an urgent need of reliable diagnostic tests for all branches of the subject? Finally, teachers should consider the possibility of introducing the usable parts of the calculus, even if the methods were not strictly rigorous. Reference was here made to the work of Mr. J. A. Swenson of New York, whose work with unpromising material, consisting largely of girls of mixed parentage, was outstanding.

At the meeting on 27th October, at Bedford College, Mr. A. W. Riley, Headmaster of Stroud Central School, spoke on " Teaching Plan and Elevation to beginners in Geometry ". There was an attendance of about fifty. Mr. Riley stated that the purpose of this work was to develop intuitive knowledge of

THE LIBRARY.

160 Castle Hill, Reading.

The Librarian reports gifts as follows:

From Prof. **E. H. Neville,** on the conclusion of his period of office as President:

J. E. Montucla	Histoire des Mathématiques (4 vols.) - -	1758, 1802

A mixed set, the first two volumes belonging to the original edition, the last two to the edition with which the author was occupied at the time of his death; of this second edition, the first two volumes were issued by Montucla in 1799, and the other two were completed by J. J. de la Lande and issued in 1802. The set is bound uniformly and appears to have been constituted as at present for a long time; among former owners were H. C. Schjellerup and J. L. E. Dreyer, and there are marginal notes by these two.

From Sir **A. S. Eddington:**

C. Huygens	Œuvres Complètes. XVII, XVIII - -	1932, 1934

From Miss **H. P. Hudson,** two runs:

Proceedings of the London Mathematical Society. Ser. 2. 26 - 1927

The set is now complete from the beginning of the original series in 1865 to the present day.

Rendiconti del Circolo Matematico di Palermo. 51 - - - - 1927

The Library now lacks 1-25 *and* 45-50.

From Mr. **B. J. Lewsley,** a collection of textbooks, an odd volume of W. Hooper's *Rational Recreations,* and

S. Brodetsky	Nomography (2) - - - - - - - - -	1925
O. Gregory	Astronomy - - - - - - - - - -	1802
A. Ingram	Concise System of Mathematics (5: J. Trotter) - -	1840
M. Somerville	Connexion of the Physical Sciences (6) - - -	1842

From Prof. **E. H. Neville:**

O. Bolza	Vorlesungen über Variationsrechnung. I - -	1908

The work was issued in three parts, of which the second and third were given to the Library by Mr. Greenstreet in 1923 and the first is out of print. To come across an odd copy of the first is a pleasant reward for keeping one's eyes open.

From the Cambridge Training College, through Miss **M. E. Bowman,** a collection of schoolbooks.

The following have been bought:

W. H. Drew	Conic Sections (3) - - - - - - - -	1864
H. J. Hose	The *Elements* of Euclid - - - - - - -	1853

"A new text, based on that of Simson."

The Leeds Correspondent. I-V - - - - - - - - - 1814-1823

A complete set of the 19 numbers of this "literary, mathematical, and philosophical Miscellany", which is No. 32 in Prof. Archibald's article in the *Gazette* 200th number. Prof. Archibald's footnote on p. 392 should be corrected: the passage in which the *Enquirer* is said to have been published "till May last" occurs in the Preface *in a quotation* from the statement in which the editor and proprietor of the *Correspondent* announced their intentions of publishing the work; since the first number of the *Correspondent* appeared in January 1814, "May last" at the time of the statement was May 1813, although "May last" at the time of the Preface was two years later.

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